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REVIEW

In Defense of Population Theory

*Robert Gutman*

The Demographic Transition

*William Petersen*

Migration, Population, and Distance

*Charles T. Stewart, Jr.*

Urban Structure and Industrialization

*Thomas O. Wilkinson*

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The Concept of Social Mobility

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# AMERICAN SOCIOLOGICAL REVIEW

June 1960

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## IN DEFENSE OF POPULATION THEORY \*

ROBERT GUTMAN

*Rutgers University*

*The thesis of this paper is that sociologists have been in error when they speak of population study as lacking in theory. On the contrary, as an attempt to classify population theories reveals, population study is rich in theoretical formulations. These theories deal with three kinds of problems and phenomena: the impact of population on productivity and economic growth; the "population problem;" and demographic phenomena per se. The theories exhibit different degrees of comprehensiveness and are expressed at various levels of abstraction. It is suggested that the claim of some sociologists that demography lacks theory is a consequence of the particular definition of theory that has become fashionable in sociology, a definition that should be replaced by a pluralistic conception of theory.*

SOCIOLOGISTS often complain that demography is too empirical. They intend many things by this accusation, one of which surely is that demography is lacking in theory. The complaint always has surprised me, given the variety of theoretical work relating to population, including mathematical theory of the sort which only now is developing in other fields of sociology.\*\* Indeed, demography has been so rich in theoretical activity that the problem for most demographers has not been to find useful theories but rather to discover some simple and convenient way of classifying the theoretical resources of the field. How is one to discuss population theory intelligently when we still lack an adequate understanding of what population theory *is*? Yet the attempt to formulate a simple classification of population theory repeatedly encounters obstacles. Some of them are similar to the difficulties which Merton reports in his important paper

on types of sociological theory,<sup>1</sup> and I have profited a good deal from the approach that he developed to overcome these obstacles.

One source of difficulty in any attempt to classify population theory is the great variety of problems and phenomena with which population theories deal. The theory of the optimum population aims to answer the question of the size or density of population that will yield, say, maximum per capita productivity, given stated conditions of technology and other resources. Many sociologists will tend to dismiss this interest as not an appropriate concern of population theory in the sense in which they prefer to use the term, for the reason that optimum theory is not interested primarily in the factors which influence population but attempts rather to study the impact of population on other social and economic variables. But population theory of this sort has a long and distinguished history. Most pre-Malthusian doctrines of population—such as those found in the writings of the French mercantile economists—dealt with

\*A revised version of a paper read at the annual meeting of the American Sociological Association, September, 1959.

\*\*For an example in this issue of the *Review*, see Charles T. Stewart, Jr., "Migration as a Function of Population and Distance" (pp. 347-356).—*The Editor*.

<sup>1</sup>Robert K. Merton; "The Bearing of Sociological Theory on Empirical Research" in *Social Theory and Social Structure*, Glencoe: Free Press, 1949, Chapter 2.

the issue of the population size that would maximize national wealth.<sup>2</sup> Malthus' *Essay on Population*<sup>3</sup> was conceived in the context of this issue, although his principal intention was to explain the existence of poverty rather than to account for wealth. The classical economists who succeeded Malthus continued to use the "principle of population" in this way. For instance, in Mill's *Principles of Political Economy* the discussion of the law of population is found in the section treating the theory of production, under the heading of labor—labor, of course, comprising along with land and capital, the three factors of production.<sup>4</sup> Not until the time of Jevons, however, was it recognized explicitly that the nature of the economist's interest in population theory differs from the usual concern of the sociological student of population. In Jevons' major opus, *The Theory of Political Economy*, he makes the following statement bearing on this issue:

The doctrine of population . . . forms no part of the direct problem of Economy . . . it is a total inversion of the problem to treat labor as a varying quantity, when we originally started with labor as the first element of production, and aim at the most economical employment of that labor. . . . It is what mathematicians would call a change of the variable, afterwards to treat that labor, which is first a fixed quantity, as a variable.<sup>5</sup>

A second subject of population theory has been the "population problem": how to explain the existence, now or in the future, of overpopulation or underpopulation in a society. Examples of population theories of this type are the Malthusian principle itself, Henry George's explanation of overpopulation in terms of the theory of rent,<sup>6</sup> and Marx's law of population in capitalist soci-

ety.<sup>7</sup> The fecundity cycle theory which Gini formulated is an example of a theory of underpopulation.<sup>8</sup>

Theories which deal with the population problem imply a definition of what constitutes a "normal" or "proper" population for the particular society in question, but the definition usually is not spelled out. Although some commentators on Malthus' earliest writings have tried to suggest that he had in mind a concept similar to the modern idea of the optimum population,<sup>9</sup> in fact what Malthus seems to have wanted was a population that would avoid the mortality produced by the positive checks and which would remain close to, or slightly above, the level of subsistence at the same time. Marx accused capitalist society of fostering overpopulation which, he asserted, is a population greater than that which fulfills the "average needs of capitalism for self-expansion,"<sup>10</sup> but he failed to offer a precise definition of the size of the population that would fulfill these needs in a particular society.

The absence of discussion of the concept of the normal or proper population for a society in theories dealing with the population problem can be understood by recalling the intent of these theories. Many of them were polemical principles designed to support specific political philosophies, as were the theories presented in the first edition of Malthus' *Essay* and in Godwin's *Political Justice*;<sup>11</sup> or they were designed to counter popular misunderstandings of the population problem, as in the work of George; or, like Marx's theory, helped to provide the rationale for a revolutionary program. Had the authors raised the issue of the nature of a "normal" population, the question would have diminished the appeal of their theories to the lay audiences they were addressing.

<sup>2</sup> Joseph Spengler, *French Predecessors of Malthus*, Durham: Duke University Press, 1942, *passim*.

<sup>3</sup> Thomas Malthus, *An Essay on Population*, London: Johnson, 1798. A good review of the connection between Malthus' principle and his ideas on the problem of poverty is contained in Kenneth Smith, *The Malthusian Controversy*, London: Routledge and Kegan Paul, 1951, Book IV, Chapter III.

<sup>4</sup> John Stuart Mill, *Principles of Political Economy*, London: Porter, 1857, Book I, Chapter 10.

<sup>5</sup> W. Stanley Jevons, *Theory of Political Economy*, London: Macmillan, 1871, pp. 254-255.

<sup>6</sup> Henry George, *Progress and Poverty*, New York: Appleton, 1881, Book II, Chapter 3.

<sup>7</sup> Karl Marx, *Capital*, London: George Allen and Unwin, 1928, pp. 697-716.

<sup>8</sup> Corrado Gini, "The Cyclical Rise and Fall of Population," in *Lectures on the Harris Foundation, Population*, Chicago: University of Chicago Press, 1930, pp. 1-140.

<sup>9</sup> E. F. Penrose, *Population Theories and Their Application*, Stanford: Food Research Institute, 1934, pp. 28ff.

<sup>10</sup> Marx, *op. cit.*, p. 300.

<sup>11</sup> William Godwin, *An Enquiry Concerning Political Justice, and its Influence on General Virtue and Happiness*, London: Robinson, 1793, 2 vols.



Theories which deal with the population problem have another distinguishing feature: although they usually point to the economic and social consequences of population size or population growth, they often do so without accounting for changes in fertility and mortality variables independently. Instead, these theories tend to discuss the size of a population and its rate of growth as if these were single unitary phenomena which respond directly to social and economic forces. Marx's theory illustrates this particular characteristic. In his discussion of the law of population in capitalist society, in *Capital*, in only one passage does he deal with the question of the impact of the means of production on the birth rate and only a few times mentions the importance of mortality as an independent factor. The burden of his argument is that the development of capitalism brings about a decline in the effective demand for labor. According to Marx's discussion, overpopulation is the consequence of this decline rather than of changes in demographic variables.<sup>12</sup>

Today, of course, theories concerned with the population problem treat their subject with greater sophistication. Although they maintain their normative intention, advances in economic analysis and in popular knowledge of economic processes, as well as changes in the audience for writings in this field, have been such that the injunctive argument rarely is used without the accompaniment of a scholarly presentation of optimum theory.<sup>13</sup> Furthermore, the contributions of fertility and mortality trends to the population problem are considered separately. Thus, a neo-Malthusian writer such as Vogt claims that population growth must be limited by birth control *because* an uncontrolled increase in fertility, combined with declining mortality, would produce a population which exceeds the optimum size.<sup>14</sup>

It is worth noting that the confusion which now prevails in both sociology and demography regarding a classification of popu-

lation theory is made more emphatic because the standard classification defines it principally with reference to theories of the population problem. I refer to Thompson's distinction between "natural" and "social" theories of population.<sup>15</sup> If one examines the theories which Thompson groups under these two classes it is apparent that they differ, in his view, because some assert or assume that the population problem is rooted in the nature of man and therefore is not solvable by changes in human institutions, whereas others argue that measures can be taken by societies to rid themselves of the problem. If some of these same theories, however, were viewed as purporting to explain a phenomenon like population size or fertility, they would have to be reclassified. For instance, Thompson regards Malthus' principle of population as a "natural" theory because Malthus originally argued that the population of any society would inevitably press upon the means of subsistence and consequently that poverty and the population problem would be with us forever. But while he did assert these pessimistic views in the first edition of the *Essay on Population*, Malthus also believed that social and economic factors, especially the food supply and subsistence, were the principal determinants of population growth—or, as he called them, the ultimate check to population. From this perspective, clearly Malthus should be classified as a "social" theorist.

The third focus of population theory is that which most often marks sociological discussion in the field, namely, the explanation of demographic phenomena *per se*. There are four groups of phenomena with which this type of theory deals: population size; the rate of population growth; the primary population processes or variables (fertility, mortality, and migration); and population characteristics, such as the age, sex, or marital composition of the population. Of these groups of phenomena, there is least theorizing with regard to the explanation of population size. The factors which influence population size are so numerous and varied in character that to formulate a meaningful theory requires a model which is more ad-

<sup>12</sup> Marx, *op. cit.*, pp. 697-716, *passim*.

<sup>13</sup> See Ansley J. Coale and Edgar M. Hoover, *Population Growth and Economic Development in Low-Income Countries*, Princeton: Princeton University Press, 1958, pp. 18-19.

<sup>14</sup> William Vogt, *Road to Survival*, New York: Sloane, 1948, pp. 279-283.

<sup>15</sup> Warren S. Thompson, *Population Problems*, 4th edition, New York: McGraw-Hill, 1953, Chapter 3.

vanced than contemporary social science makes available. Furthermore, attempts to explain population size except in terms of sets of birth, death, and migration events, and the factors influencing these sets of events, have failed to excite the interest of demographers. Perhaps the best illustration of this is the negligible impact of Carr-Saunders' peculiar version of optimum theory on later theorists.<sup>16</sup> And probably even Carr-Saunders himself was able to conceive a theory in which population size was regarded as a single unitary phenomenon reacting directly to the physical environment and social structure only because he chose to apply his theory to pre-literate cultures about which there were so few data that they did not reveal immediately the hopelessness of the enterprise.

Transition theory<sup>17</sup> dominates contemporary thought concerned with the rate of population growth, but there is another approach bearing on this same subject, manifest in formal analyses having their source in Lotka's stable population theory and the logistic curve hypothesis discussed by Pearl and Reed.<sup>18</sup> Both approaches to the interpretation of growth developed, in turn, from the researches of nineteenth century economists, mathematicians, and statisticians. Transition theory is a modern elaboration of Mill's distinction between Malthusian and non-Malthusian populations,<sup>19</sup> whereas the work of Pearl and Reed represent a rediscovery of

principles first enunciated by Verhulst.<sup>20</sup> The number of generalizations to explain population processes is legion, with most of them concentrated in the area of fertility, several in migration, but only a few in mortality.<sup>21</sup> Many population characteristics about which information is collected in census tabulations are relevant also to the analysis of social structure; consequently, it turns out that some of the best work in this field of population theory has been done by students of the family, social stratification,<sup>22</sup> and other specialties within general sociology. A good deal of research is carried on by demographers themselves, however, about the factors which influence the sex and age composition of the population, and formal demographic analysis designed to estimate the future character of the population with respect to these features is a viable area of population theory.

#### ABSTRACTION AND COMPREHENSIVENESS IN POPULATION THEORY

The ambiguity in the terms "population theory," in the sense in which this ambiguity has been discussed so far, does not stem, as is the case with much sociological theory, from the complexity or vagueness of the

<sup>16</sup> The theory is expounded in A. M. Carr-Saunders, *The Population Problem*, London: Oxford University Press, 1922.

<sup>17</sup> A review, critique, and bibliography of transition theory are found in Philip M. Hauser and Otis D. Duncan, *The Study of Population*, Chicago: University of Chicago Press, 1959, pp. 93-96 and 104-105. (For a critique in this issue of the *Review*, see William Petersen, "The Demographic Transition in the Netherlands," pp. 334-347.—*The Editor*.)

<sup>18</sup> The fullest statement of Lotka's theory can be found in his *magnum opus*, A. J. Lotka, *Théorie analytique des associations biologiques. Part I. Principes. Part II. Analyse démographique avec application particulière à l'espèce humaine*, Paris: Herman, 1934-1939. The researches of Pearl and Reed first appeared in 1920. See Raymond Pearl and Lowell J. Reed, "On the Rate of Growth of the Population of the United States Since 1790 and Its Mathematical Representation," *Proceedings of the National Academy of Sciences*, 6 (1920), pp. 275-288.

<sup>19</sup> Mill, *op. cit.*, Book I, pp. 193-197.

<sup>20</sup> Pierre F. Verhulst, "Notice sur la loi que la population suit dans son accroissement," *Correspondence mathématique et physique publiée par A. Quetelet*, Brussels: 1838, X, pp. 113-121.

<sup>21</sup> Kingsley Davis, "The Sociology of Demographic Behavior," in Robert K. Merton et al., editors, *Sociology Today*, New York: Basic Books, 1959, pp. 314-316.

<sup>22</sup> Numerous examples of sociological writings could be cited to illustrate this statement. In the field of stratification see, for instance, Natalie Rogoff, *Recent Trends in Occupational Mobility*, Glencoe: Free Press, 1953. Parsons' work on the family approaches an interest in demographic issues. See Talcott Parson and Robert F. Bales, *Family, Socialization and Interaction Process*, Glencoe: Free Press, 1955, Chapters 1 and 2.

The effort of some demographers to restrict the study of population characteristics to demography is illustrated by the unorthodox definition of demography stated recently by Hauser and Duncan: "Demography is the study of the size, territorial distribution and composition of population, changes therein, and the components of such changes, which may be identified as natality, mortality, territorial movements (migration), and social mobility (change of status)." Hauser and Duncan, *op. cit.*, p. 2 (my italics).



phenomenon which is being examined. It is often difficult to know what different sociologists mean by such words as "attitude," "reference group," "role expectation," or the like, and one can doubt whether the phenomena indicated by these concepts are adequately represented in the operational definitions which guide the research conducted in terms of the concepts. But there is rarely corresponding confusion about the meaning of terms like "mortality," "fertility," or even "the population problem" in discussions of population theory. These concepts have a concreteness and definiteness which are seductive to the student acquainted with other fields of contemporary sociology. The ambiguity discussed above has a simpler source: population theory is an expression which has been used to denote explanations dealing with a variety of problems and phenomena. There is another type of ambiguity in population theory, however, which matches that found in most sociological theory. The source of confusion here is the meaning of the word "theory." In practice, generalizations on several different levels of abstraction with varying degrees of comprehensiveness are labeled population theory.

The term "theory" has been used as a synonym for a great number of concepts used to organize population data. The task of analyzing the assumptions and implications of these concepts, and refining them, has been considered a kind of theoretical activity. These concepts generally have been of three varieties: those dealing with a notion of the optimum population; concepts which might better be considered indices, presuming to measure static or dynamic aspects of population phenomena, such as the crude birth rate or the net reproduction rate; and concepts in terms of which changes in population growth are explained: urbanization, industrialization, and socio-economic status.

With respect to the concept of the optimum population, the problem for the theorist has been to find an index for the criterion of the optimum which both captures the sense of the concept and is operationally meaningful. Several indices meeting these qualifications have been suggested, although no single index is in use among all demographers. It should be noted that the extensive literature on the theory of the optimum population is con-

cerned as often with finding an appropriate index as with examining the assumptions that underlie the theory as a whole; in other words, writings on the theory of the optimum sometimes are confined exclusively to the analysis of concepts.<sup>23</sup>

Of all the standard measures of population growth, the net reproduction rate has undergone the most severe criticism in recent years.<sup>24</sup> This index expresses the extent to which a population would eventually replace itself, assuming current age-specific fertility and mortality rates remain unchanged. The rate was formulated in order to eliminate the biases produced in indices such as the crude birth rate or the crude rate of natural increase by temporary abnormalities in the age composition of the population. Critical commentary dealing with the net reproduction rate has now made demographers more aware than they were formerly of the several biases that it, too, contains. The rate sometimes was used to estimate future growth on the assumption that present vital rates would persist in the immediate future, an assumption which proved untenable, given the experience of Western nations in the post-war years. Although the net reproduction rate presumes to correct the bias resulting from variations in the age composition of populations, the effect of compositional factors is not wholly removed, for the reason that the rate assimilates the influence of variations in the marital composition of populations. So many shortcomings of the measure have been noted that it is gradually disappearing from professional literature on fertility and population growth, but textbooks in population still give it considerable attention.<sup>25</sup>

The concepts which describe the factors influencing population size and growth are beginning to become the subject of theoretical analysis. Probably a great deal more interest will be shown in this area of population theory in the current decade as American demographers conduct more research on

<sup>23</sup> Joseph Spengler, "Population Theory" in Bernard F. Haley, editor, *A Survey of Contemporary Economics*, Homewood, Ill.: Irwin, 1952, pp. 111-114.

<sup>24</sup> One of the best summaries of the critiques is by George Stolnitz and Norman Ryder in "Recent Discussion of the Net Reproduction Rate," *Population Index*, 15 (April, 1949), pp. 114-128.

<sup>25</sup> E.g., Thompson, *op. cit.*, Chapter 8, *passim*.

the problems of the underdeveloped nations of the world and seek to explain and predict the consequences of modern civilization for population growth in Asia, Africa, and Latin America. The immediate and primary need is to restate these concepts so that they can be modified by empirical research, in the way, for example, Moore has done in his study of Mexico of the "kinds of circumstances [which] induce workers to leave traditional modes of production and enter modern economic activity."<sup>26</sup>

A second type of generalization was first classified by Lorimer in a paper dealing with social science theory in general. Under the heading of population theory, he included "a growing body of appropriate mathematical and logical formulations for the guidance and interpretation of research,"<sup>27</sup> as an instance of which he cited the theory of the stable population. Analytic propositions of this type, Lorimer pointed out, have the advantage of being "rigidly scientific, . . . any two workers using the same logical apparatus in relation to the same observations must reach identical conclusions."<sup>28</sup>

Although some concepts, such as the net reproduction rate, possess features of analytic propositions, propositions generally lead to the recognition of more new formal relationships than are revealed by concepts. This certainly seems to be true of stable population theory compared to the net reproduction rate. On the other hand, analytic propositions share some of the defects of concepts. Stable population theory is most useful as a device to predict future events such as age composition or fertility if the present annual rate of increase and the age-specific fertility and mortality rates of an actual population continue for at least two generations. Such a high degree of demographic stability, however, occurs rarely among modern industrialized populations. Therefore, one of the major tasks of population theory—to anticipate vital rates—usually cannot be handled by the propositions relating to the stable population; although, interestingly enough, where

the age composition of an actual population is fairly stable, as in India, stable population theory remains a remarkably effective tool of demographic analysis.<sup>29</sup>

In fairness to those who work in the area of formal analytic theory, it should be noted that the authors of stable population theory probably never intended that it should be used to forecast events in actual populations. Hajnal discusses this point in the course of distinguishing between "ambitious analytic theory," such as the logistic curve, and what he calls "simple analytic models," such as stable population theory:

In the ambitious uses, the emphasis is on establishing a mathematical relationship which is believed to hold, at least approximately, in the real world. The ambitious models are intended to be plausible representations of the world which may subsequently be applied in a variety of contexts. It is not usually possible to indicate just how inadequacies in the model might affect its various applications. On the other hand, when a model is used as an analytic tool, it is being used to cope with a particular problem. In so far as the mathematical relationships, which are not obvious, are involved, their plausibility as representations of the real world is not under discussion, but only whether they are adequate for the purpose in hand. It is not necessary that such mathematical relationships should be plausible; often they are not. It is, however, necessary that the magnitude of the error resulting from the discrepancy between model and fact can be assessed (though, of course, this assessment need not be made explicitly).<sup>30</sup>

Techniques for projecting population trends are sometimes considered theory. They are so designated because these techniques do some of the work of theory, that is, they try to predict future developments including population size and population processes and characteristics. In most cases, these techniques involve little speculation about the nature of the phenomena involved or the forces which influence them. The method Coale developed for estimating European and Soviet mortality after World War II, for instance, depended almost exclusively upon the extrapolation of past trends by formal techniques.<sup>31</sup> Whelpton's method of population

<sup>26</sup> Wilbert E. Moore, *Industrialization and Labor*, Ithaca: Cornell University Press, 1951, p. 299.

<sup>27</sup> Frank Lorimer, "The Differentiation of Logical Levels in Social Inquiry," *American Sociological Review*, 12 (October, 1947), p. 508.

<sup>28</sup> *Ibid.*, p. 508.

<sup>29</sup> Coale and Hoover, *op. cit.*, Chapter 3.

<sup>30</sup> John Hajnal, "Mathematical Models in Demography," *Cold Spring Harbor Symposium on Quantitative Biology*, 22 (1957), p. 101.

<sup>31</sup> F. W. Notestein, I. B. Taeuber, D. Kirk, A. J. Coale and L. K. Kiser, *The Future Population of*

forecasting, a variant of which is still used by the Bureau of the Census in constructing population projections for the United States, is somewhat more "theoretical."<sup>32</sup> The Bureau's projections are prepared by first estimating future values of fertility and mortality, after which trend lines are fitted to connect these estimated future vital rates with present and past rates. In estimating future rates, certain guesses are made which involve assumptions about the social and economic factors which influence population processes and the condition of these forces in the future. It should be noted, however, that whatever theorizing is done is implicit. There are demographers who believe that the lack of an explicit interest in theory has been the crucial source of inaccuracy in Census Bureau projections over the last two decades. But it is hard to see why theory necessarily would have anticipated the decline in the age of marriage and in the age of the mother at the birth of the first child. These two demographic changes have been major factors contributing to the rise of the American birth rate since World War II, and the rise in the birth rate, in turn, was the principal variation not anticipated by the Census Bureau projections.

Merton has written of sociological theory: "Much of what is described in textbooks as sociological theory consists of general orientations toward substantive materials. Such orientations involve broad postulates which indicate types of variables which are somehow to be taken into account rather than specifying determinate relationships between particular variables."<sup>33</sup> Examples of general orientations abound in the literature of population theory. Gini emphasized fecundity as a determinant of population growth, asserting that changes in the capacity to reproduce from generation to generation account for the rise and decline of population;<sup>34</sup> whereas Malthus explicitly denied the significance of this variable. "It is probable," Malthus wrote, "that the natural prolificness of women is

nearly the same in most parts of the world."<sup>35</sup> In place of the concept of variations in fecundity, he urged that the means of subsistence is the important variable influencing population size. As Davis has demonstrated, most of the propositions which make up the totality of Malthusian theory can be attributed to the choice of this general orientation.<sup>36</sup> Malthus' orientation, of course, underlies most contemporary theory about population, regardless of the level of generalization, whereas Gini's point of view is discredited.

Several theoretical keystones of contemporary population study are really empirical generalizations; that is to say, they describe "a set of uniform conjunctions of traits repeatedly observed to exist, without any understanding of *why* the conjunction occurs; without a theory which states its rationale."<sup>37</sup> Transition theory and generalizations about the inverse relationship between fertility and socio-economic status probably belong in this category, even though demographers do have positive notions about why the demographic transition has taken place and the reasons which explain the higher fertility among the poor. But they are *notions* rather than carefully conceived and well-reasoned explanations.<sup>38</sup>

Transition theory achieved its modern form in the work of Thompson,<sup>39</sup> after international population statistics had become sufficiently accurate to enable a demographer to undertake a review of the condition of the world's population. Thompson's aim was

<sup>32</sup> Thomas Malthus, *Essay on the Principle of Population*, London: 1817, 5th edition, II, p. 139.

<sup>33</sup> Kingsley Davis, "Malthus and the Theory of Population," in P. F. Lazarsfeld and M. Rosenberg, editors, *The Language of Social Research*, Glencoe, Ill.: Free Press, 1955, pp. 540-553.

<sup>34</sup> John Dewey, *Logic: The Theory of Inquiry*, New York: Holt, 1938, p. 193.

<sup>35</sup> For the rudiments of a discursive explanation of the demographic transition, see Kingsley Davis, *Human Society*, New York: Macmillan, 1949, pp. 599-600. In very recent years, a systematic explanation of differential fertility has been attempted by Mishler and Westoff. See Elliott G. Mishler and Charles F. Westoff, "A Proposal for Research on Social Psychological Factors Affecting Fertility: Concepts and Hypotheses," in *Current Research in Human Fertility*, New York: Milbank Memorial Fund, 1955, pp. 121-150.

<sup>36</sup> Warren S. Thompson, "Population," *American Journal of Sociology*, 34 (May, 1929), pp. 959-975.

*Europe and the Soviet Union*, Geneva: League of Nations, 1944, pp. 183-189.

<sup>32</sup> Pascal K. Whelpton, *Forecasts of the Population of the United States, 1945-1975*, Washington, D. C.: U. S. Government Printing Office, 1947, pp. 3-16.

<sup>33</sup> Merton, *op. cit.*, p. 85.

<sup>34</sup> Gini, *op. cit.*, pp. 3-16.

to classify world populations in terms of their rates of growth. Trying to explain the variation in growth rates, he was led to conclude that these changes are correlated with industrialism, and to suggest that underdeveloped countries would, as they became industrialized, adopt the characteristics of the Western nations. Thompson's hypothesis was partly confirmed by the demographic history of the subsequent decade. During the 1930s, it was discovered that Japan and Russia, which he had grouped together as Class C nations—exhibiting high birth rates and high death rates—now had become Class B countries with the onset of industrialization, exhibiting continued high birth rates but lower death rates. Such observations lay behind Notestein's much more precise description of population types in 1945, and the latter's use of the term "demographic evolution" to describe the tendency of nations to pass from a condition of "high growth potential" through a period of rapid population growth to a condition of "incipient decline."<sup>40</sup> The generalization nevertheless continued to have the character of an empirical generalization, since the rationale presumably implicit in it had never been systematically stated. Indeed, the reasoning underlying the statement remains implicit at the present time, although many commentators have pointed to the need to make it explicit.<sup>41</sup> Probably the need for the statement of the rationale of the demographic transition is greater today than ever before in the theory's history, because of the renewed growth of many Western populations and the unanticipated, precipitous decline of mortality in some of the underdeveloped nations.

Societal laws differ from empirical generalizations because they are *derived* from statements on a higher level of abstraction, and are the product of deduction rather than induction. In the paper on Malthus by Davis, referred to above, he has shown that some of the theoretical statements for which Malthus is famous are really laws. For example, Malthus' contention that the positive and preventive checks to population vary inversely can be shown to follow from his

postulates that both the sexual instinct and the desire for food are necessary to man's survival and exist in all men with about equal intensity.<sup>42</sup> Probably Marx's principle of population, which asserts that the working population is turned into a relatively surplus population, is also a law in this sense, deducible from Marx's theories of value and of capitalist production. It remains a question, however, whether the process of reasoning through which Marx and Malthus arrived at laws was truly deductive, or whether instead their fundamental postulates were enunciated only after the laws themselves were formulated.

#### A PLURALISTIC CONCEPTION OF POPULATION THEORY

In view of the different concerns of population theory and the various levels of generalization in terms of which these concerns have been expressed, perhaps the reasons for the absence in the literature of demography of a simple classification of population theories become clear. But once the varieties of population theories are reviewed, it is all the more difficult to comprehend why sociologists, including some sociological demographers,<sup>43</sup> still say that demography is too empirical and is lacking in theory. The answer to this riddle is the fashion which has taken hold in sociology of reserving the label "theory" for a set of very abstract propositions which are systematically and logically related but which usually have no empirical referents, indeed have no apparent connection with society as it is. According to this conception of theory, the generalizations with empirical significance are to be deduced from the abstract propositions, converted into operational procedures, then tested, and through this process the meaningfulness of the theory itself is to be ascertained.<sup>44</sup> The question of the propriety of the sociological definition aside, the

<sup>42</sup> Davis, "Malthus and the Theory of Population," *loc. cit.*, p. 548.

<sup>43</sup> Vance is a sociological demographer who regrets the absence of "theory" in demography. See Rupert B. Vance, "Is Theory for Demographers?" in J. J. Spengler and O. D. Duncan, editors, *Population Theory and Policy: Selected Readings*, Glencoe, Ill.: Free Press, 1956, pp. 88-94.

<sup>44</sup> Cf. Robert Gutman, "Cooley: A Perspective," *American Sociological Review*, 23 (June, 1958), esp. p. 254.

<sup>40</sup> Frank W. Notestein, "Population—The Long View," in T. W. Schultz, editor, *Food for the World*, Chicago: University of Chicago Press, 1945.

<sup>41</sup> *Supra*, n. 16.



prevalence of this definition of theory points toward a third obstacle to the classification of population theory: students of population are unable to agree about what population theory *should be*, regardless of what it now is. To put it in another way, progress in sorting out population theories is held back because the various disciplines associated with demography approach the subject of population theory with differing standards and programs of how theory best can be formulated.

Economists have a preference for theory on a very general level, especially for theory which can be expressed in mathematical language. Some demographers share this preference. When they encounter problems involving sociological concepts which cannot easily be manipulated by the use of mathematical models, they forsake the problems and concentrate instead on the discovery of empirical uniformities. Other demographers, usually those who come to the study of population with a background in economics, choose to employ mathematical concepts even if recourse to models based on them forces these demographers to investigate only the formal interrelations between simple variables like age and sex. When sociologists discover that contemporary sociological theory is not directly applicable to population questions, they turn to theory borrowed from social psychology—as do most of the students of American fertility; or they hope that somebody *else* will demonstrate how functional analysis, say, can be integrated with demography.

Each of these approaches to population theory has been valuable for dealing with some set of population phenomena, but none of the types of theory used in any single discipline has been able to develop compelling or even fruitful explanations of all population phenomena, although demography has had the status of a social science for more than 150 years. Given this situation, might it not be the path of wisdom to accept a pluralistic conception of population theory? When faced with the challenge of classifying population theories, should we not define population theory simply, as “the widest body of rigorous reasoning” concerned with the impact of population variables on society;

with the solution to the population problem; and with the trend of population size, the rate of population growth, and the various population processes and characteristics? So long as the disciplines interested in demography help us to understand, explain, and predict population phenomena, does it really matter whether this reasoning is in the form of a general orientation, a conceptual analysis, an analytic proposition, a law of population, or an empirical generalization, and that there are no theories which fit the definition of theory that currently prevails in sociology?

I suppose that there are readers of this paper who would interpret a negative answer to these questions as an endorsement of persistent chaos. Without going into the issues regarding the nature of theory in social science that a proper defense against this accusation may demand, I would point out that, unfortunately, the ambitions for developing statements that conform to the ascendant sociological definition of theory have not been fulfilled. Grand programs for constructing theories have not enlarged our knowledge and understanding of society nor have they proved to be reliable guides for social research. We are rich in methodological prescriptions for the form that theory should take if social theory could take such a form, but apparently theories about society having any empirical content must be sought by the use of other intellectual means. As a consequence, what passes for theory in much of sociology today is “a long stretch of purely verbal analysis.”<sup>45</sup> In the meantime demography, which so far has avoided the self-consciousness and methodological parochialism of much contemporary sociology, continues to offer illuminating theoretical statements which organize existing knowledge, lead to the acquisition of new knowledge, and help in the solution of population problems.

<sup>45</sup> Davis, “The Sociology of Demographic Behavior,” *loc. cit.*, p. 313. Davis’ statement is worth quoting in full: “In social science, [theory], instead of meaning the widest body of rigorous reasoning about a set of observed relationships, has come to mean a long stretch of purely verbal analysis. If a publication contains any empirical evidence, particularly of a statistical kind, it is not theory; but if it contains only verbal generalizations, no matter how loosely connected, it is theory.”

## THE DEMOGRAPHIC TRANSITION IN THE NETHERLANDS \*

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*This paper is an attempt to relate the broad theory of the demographic transition to the specific population history of one country, insofar as this can be ascertained from incomplete and faulty data. Four conclusions are drawn: (1) The population of the area presently defined as the Netherlands has undergone a large more or less continuous increase since the beginning of the modern era. (2) Net migration probably had little effect on population growth. (3) The decline in mortality was not sufficient before the last decades of the 19th century to account for the increase in numbers. (4) There was a rise in fertility, following the breakdown of institutional and moral inhibitions to procreation.*

OFFHAND one might suppose that demography ought to be one of the happiest meeting-grounds of sociologists and historians. Studying the population of any society is typically begun with a social analysis—comparing the fertility, say, of one class with that of another. And in this case such an analysis is much less likely to be static than is usual in other branches of sociology, for both the flow of life from one generation to the next and the succession of censuses suggest, and sometimes demand, a historical framework. Actually, however, demography has not benefited from very much interpenetration of the two disciplines. Most of the historians seriously concerned with population have concentrated on the period before the advent of reliable statistics, and their demographic expertise has not ordinarily been at a high level. And sociologists have usually been content with that roughest and most simplistic of models, the theory of the demographic transition, the bare bones of which have all too seldom been rounded out with historical detail.

This at least was the situation until rather recently. But the last decade or so has seen a relatively large number of excellent studies, which in sum may eventually revise our ideas of population trends in the early modern period. Perhaps the most important change being made in the conception of the demographic transition is in the refinement of the

original thesis that it applies equally well to all countries undergoing modernization. It has by now become obvious that there are more differences than similarities in the population development of, say, 19th century England and 20th century India. Several writers have suggested that among Western cultures overseas countries like the United States and the British dominions, whose empty lands were filled in large part by immigration, constitute a special subclass. Even population growth in so homogeneous a culture area as Western Europe has differed significantly from one country to another, and in order to obtain a true picture of its demographic past it will be necessary to undertake many more national or even local historical studies and at some future time to synthesize them into a more complex, but more accurate, over-all model.

In this paper I wish to bring to the attention of their English-speaking colleagues some of the interesting and important work that Dutch sociologists and historians have been doing in population analysis, and to suggest a few of the general theoretical implications of their findings. That the population history of the Netherlands is anomalous is of course well known, but some of its specific features seem also to be variations on Western themes. If the relation between population growth and modernization is to be better understood, we must learn how to maintain a delicate balance between the specific facts of the historian and the generalizing function of the sociologist.

### POPULATION GROWTH

In the theory of the demographic transition, one postulate is that the population

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growth was wholly a natural increase; and before we apply this theory to any specific historical case we must ask whether the net migration actually was insignificant. This was the case in the Netherlands during the 19th and 20th centuries, to the extent that one can tell from the inadequate statistics.<sup>1</sup> But what of earlier centuries? When the Republic of the United Netherlands was established in the 16th century, it became a haven for refugees from Catholic Europe, first of all Calvinists from the reconquered Spanish Netherlands (now Belgium) and Jews from Spain itself, later French Protestants and East European Jews. No accurate record was kept of this immigration,<sup>2</sup> nor of the emigration that partly balanced it. That the Spanish Jews and Huguenots had a great impact on Dutch commerce and industry is not a good clue to their numbers, particularly when these are to be taken as a percentage of the national population rather than of the relatively few towns where the immigrants mainly settled. Whether the zero net migration that the model demands was true of the Netherlands is not known, but it is reasonable to assume that it was.

Estimating internal migration is no less difficult, and the problem cannot be wholly bypassed. The best of the early data refer principally to the urban population, and in order to use them for our purpose we must try to distinguish between the natural increase of the towns and the net migration to

them. During the three centuries or so preceding the first national census, the towns' population increased greatly,<sup>3</sup> but certain of the data suggest that this was the consequence mainly of large in-migration.<sup>4</sup>

Sometimes it is possible to check this impression by relating the population growth of the towns to that of the countryside by the use of provincial censuses. In the case of Holland Province, for example, we can compare the urban and rural sectors in 1622<sup>5</sup>

<sup>3</sup> Around 1500, which is almost as far back as the first records will take us, the largest city, Utrecht, had fewer than 20,000 inhabitants. Five or six others—in order of size, Leiden, Delft, Haarlem, Amsterdam, Gouda, and Dordrecht—had more than 10,000 each, and a half dozen others something under this figure. About 1550, Amsterdam and Utrecht each had about 35,000, four other cities about 20,000, eight others between 12,000 and 15,000. Shortly after 1600, Amsterdam had over 100,000 and was the largest city in the Low Countries, Leiden and Haarlem were almost half as large, three other towns had more than 20,000 each, four others more than 15,000. In the late 17th and 18th centuries, the urban growth was slower, and in some regions there was even a considerable decline from about 1750 on. See Roger Mols, *Introduction à la démographie historique des villes d'Europe du XIVe au XVIIIe siècle*, Gembloux: Duculot, 1954–1955, 2, pp. 520–523; Leonie van Nierop, *De bevolkingsbeweging der Nederlandsche stad*, Amsterdam: Binger, 1905; W. S. Unger, "De oudste Nederlandsche bevolkingsstatistiek," *Economist*, 62 (1913), pp. 745–764; Van Nierop, "De aanvang der Nederlandsche demographie," *Economisch-Historisch Jaarboek*, 5 (1919), pp. 192–208.

<sup>4</sup> Of the men inscribed in Amsterdam's marriage registers, for instance, 51 per cent were born outside the city during the first quarter of the 18th century, 55 per cent in 1750, 60 per cent in 1791 (*ibid.*). These very high proportions cannot be taken, however, as in-migration rates. The migrants were undoubtedly mostly young adults and thus disproportionately represented among bridegrooms, and whatever out-migration from the city took place usually escaped being recorded. In any case, Amsterdam was not typical of Dutch cities, nor is it today. See also Van Nierop, "Het zielental van Amsterdam in het midden van de achttiende eeuw," *Amstelodamum*, 38 (1951), pp. 151–154, where data of the same type are used to argue that the city's natural increase during the 18th century was nil, so that both the growth and the later decline were the consequence of migration.

<sup>5</sup> The census of 1622 was taken to prepare for the levy of a special head tax, and earlier analysts have for this reason rejected it out of hand. But Van Dillen, who has made the most detailed study of this count, believes that the underenumeration typical of fiscal censuses was less serious than in most others, because in this case the administra-

<sup>1</sup> See William Petersen, *Planned Migration: The Social Determinants of the Dutch-Canadian Movement*, Berkeley: University of California Press, 1955, Chapter 3.

<sup>2</sup> For example, Amsterdam maintained a "dénombrement de tous les Protestants réfugiés" from 1681 to 1684, but discontinued it just before the revocation of the Edict of Nantes and the consequent much larger migration. During these three to four years, almost 2,000 persons were listed. See the discussion in J. G. van Dillen, "Omvang en samenstelling van de bevolking van Amsterdam in de 17e en 18e eeuw," *Bijdragen en Mededelingen der Dialecten-Commissie van de Koninklijke Nederlandse Akademie van Wetenschappen te Amsterdam*, XIV, *Bevolking en taal van Amsterdam in het verleden*, Amsterdam: Noord-Hollandsche Uitgevers Maatschappij, 1954, pp. 1–24. During the 18th century, many of the Sephardic (so-called "Portuguese") Jews left Holland, and were replaced by Ashkenazi Jews from Germany and Lithuania; see Ernst Baasch, *Holländische Wirtschaftsgeschichte*, Jena: Gustav Fischer, 1927, pp. 251–252.

TABLE 1. POPULATION OF HOLLAND PROVINCE, 1622 AND 1795

	Towns	Countryside	Total
1622	397,882	269,698	667,580
1795	518,561	258,005	776,566
Per cent increase	+ 30	- 5	+ 16

and in 1795, as shown in Table 1.<sup>6</sup> The increase of 16 per cent over 175 years means an average of less than 0.1 per cent per year, but this figure can be accepted only with three reservations: (1) If we substitute for the two actual census counts the larger figures including underenumeration as estimated by Van Dillen, the increase would be by 25 rather than 16 per cent. (2) The calculation of the trend between the two censuses by simple subtraction blurs the fact that the growth curve for both towns and countryside rose during the 17th and early 18th century and fell off sharply from about 1750. (3) Neither the towns nor especially the rural regions of Holland Province were typical of the Netherlands as a whole.

This last point is worth expanding. In their generalizing function, sociologists tend to structure any analysis of town and country in the early modern period into a fairly rigid functional division between agricultural and nonagricultural localities, and thus between *Gemeinschaft* and *Gesellschaft*. The historical example of Holland Province suggests that in any particular case this can be a gross oversimplification. Its rural economy, according to De Vooy, included the following quite heterogeneous and sometimes dynamic elements: (1) *Agriculture*. In some areas this sector of the economy and the population based on it were relatively static. But in the so-called Westland—the strip along the coast

tion was exceptionally efficient. The province was divided into 23 localities, each under a special commissioner who directed the precinct officials in the towns and the sheriffs in the countryside, and both of these latter groups were required to take a special oath of office. Moreover, even at this early date, the tax was a progressive one, adjusted to both the payer's income and the size of his family. See J. G. van Dillen, "Summiere staat van de in 1622 in de Provincie Holland gehouden volkstelling," *Economisch-Historisch Jaarboek*, 21 (1940), pp. 167-189.

<sup>6</sup>A. C. de Vooy, "De bevolkingsverspreiding op het Hollandse platteland in 1622 en 1795," *Tijdschrift van het Koninklijk Nederlandsch Aardrijkskundig Genootschap*, 70 (1953), pp. 316-330.

south from The Hague, which to this day is the center of commercial horticulture—the intensification of agriculture afforded a base for population increase during the 17th and 18th centuries. (2) *Peat-cutting*. With the depletion of peat bogs in South Holland, peat-workers were replaced by a smaller number of agriculturists, resulting in a population loss in this region. (3) *"Suburban" commerce and handicrafts*, particularly in the environs of Amsterdam and Rotterdam. Here the population fluctuated together with that of the cities. (4) *Fishermen and marine workers* living in both coastal and inland villages north of the IJ River. The virtual disappearance of their means of subsistence resulted in a considerable out-migration and decline in population in the second half of the 18th century.

That the urban population of Holland Province increased by 30 per cent over the designated period while the rural population fell off by five per cent was undoubtedly due in large part to migration, both between these sectors and from other provinces to Holland's cities. But in view of the atypical features of Holland Province, it is well to check this conclusion with data from Overijssel, a generally agricultural province with some early industry. In Table 2 the population growth of this province is shown separately for the three largest towns and, below, for the smaller towns and the countryside of the three socio-economic areas of Salland, Twente, and Vollenhove.<sup>7</sup> Note that before the 19th century the rate of increase in the three large towns was well below the average for the whole province, and that by and large the growth curves of the small towns and the countryside tended to move together. These figures suggest that rural-urban migration was not so important a factor in urban growth as in Holland Province, and perhaps Overijssel was more representative of the

<sup>7</sup>B. H. Slicher van Bath, *Een samenleving onder spanning: Geschiedenis van het platteland in Overijssel*, Assen: Van Gorcum, 1957, pp. 70-71. The overlap of 16 years between columns 2 and 3 is intentional: it is not possible to fix precisely the date when the retardation in population growth began. The early population figures in this work are based on the plausible manipulation of a wide variety of local statistics. While the methodology is an interesting topic in itself, to discuss it here would take us too far afield.

TABLE 2. REGIONAL POPULATION INCREASE IN OVERIJSEL, 1675-1849

	Per Cent Increase in Population			
	1675-1723	1723-1764	1748-1795	1795-1849
Overijssel	37.6	35.9	9.5	60.4
Three largest towns (Zwolle, Deventer, Kampen)	20.7	15.8	3.1	65.8
Other towns in				
Salland	44.1	36.6	12.4	24.3
Twente	33.4	74.9	7.0	58.2
Vollenhove	37.5	4.0	13.4	60.3
Countryside of				
Salland	31.1	34.9	17.1	69.0
Twente	72.5	58.1	8.4	42.0
Vollenhove	41.6	24.6	9.7	75.7

country as a whole. It may be indicative that for the period since 1795, the growth curves of this province and of the Netherlands are almost identical.<sup>8</sup>

According to the estimate of Slicher van Bath, the population of Overijssel at the specified dates was as follows:

1475—	52,660	Earliest estimate possible; not reliable.
1675—	70,678	Earliest fairly reliable estimate.
1795—	134,104	First national census.
1840—	197,694	First reliable national census.
1957—	748,337	A recent estimate from population registers.

This steady and increasingly rapid growth does not include short-term fluctuations. A more detailed analysis<sup>9</sup> permits the population increase from 1675 to 1930 to be divided into four periods, with the average annual rates of growth as follows:

75 years (1675-1748)—	0.75 to 1.0 per cent.
60 years (1748-1811)—	zero to 0.5 per cent.
80 years (1811-1889)—	almost 1.0 per cent.
40 years (1889-1930)—	1.3 rising to 1.7 per cent.

Do these data take us back to the hypothetical Stage I of the demographic transition? The so-called static population characteristic of this stage typically fluctuates around a horizontal mean, and it would seem that this cycle is to be seen in Overijssel between 1675 and 1811. The average growth during this period, however, was not zero; this suggests that these 136 years constitute rather a transition from Stage I to Stage II. If the upswing from 1675 to 1748 was faster

than that of an ordinary Stage I cycle, as may well have been the case, this was presumably because the premodern prosperity was enhanced by the new factors that eventually would effect a steady increase in numbers. In the second half of the 18th century, a time of economic depression, the population growth slowed down and for the leanest thirty years was not much more than zero. Here again we can reasonably hypothesize that the figures reflect two overlapping curves—a decrease in population that would have resulted from a Stage I depression, cancelled by Stage II factors favoring population increase.

It seems reasonable, lacking precise data, to apply this scheme to the population history of the whole country. According to one estimate—or better, guess—the number of inhabitants of the present area of the Netherlands in 1540 was 882,400.<sup>10</sup> To specify this figure to the nearest hundred is certainly unwarranted, but it may well be correct to the nearest hundred thousand. From the middle of the 16th to the middle of the 18th century, then, the population probably grew from less than a million to something over two million, and during the second half of the 18th century it probably remained nearly static. The growth since the date of the first national census, as shown in Table 3, falls

<sup>10</sup> J. C. Ramaer, "De middelpunten van bewoning in Nederland voorheen en thans," *Tijdschrift van het Koninklijk Nederlandsch Aardrijkskundig Genootschap*, 38 (1921), pp. 1-38, 174-214. The estimate was based on counts of the number of dwellings in various towns and in the whole of Holland Province, but more than half of the total constitutes the unmeasured rural sector of the other provinces.

<sup>8</sup> See the graph in *ibid.*, p. 81.

<sup>9</sup> *Ibid.*, p. 56.

within Stage II of the demographic transition, and even within the early phase of this stage, before the long-run growth rate has begun to decline. From 1795 to 1870 the annual increase averaged about 0.75 per cent, and since 1870 it has been around 1.25 per cent. In the Netherlands, there is not only no indication of the "incipient decline" in popu-

TABLE 3. POPULATION GROWTH IN THE NETHERLANDS, 1795-1958

Year *	Population (-000)	Per Cent Average Annual Increase During Preceding Period
1795	2,097	—
1829	2,613	0.72
1839	2,861	0.91
1849	3,057	0.67
1859	3,309	0.80
1869	3,580	0.79
1879	4,013	1.14
1889	4,511	1.18
1899	5,104	1.24
1909	5,858	1.39
1920	6,865	1.45
1930	7,935	1.46
1940	8,923	1.18
1950	10,200	1.35
1958	11,278	1.25

\* As of December 31 of the designated year, except for 1849 (November 19) and 1869 (December 1). Figures from 1795 to 1930 are from the census; thereafter from the population registers.

lation characteristic of Stage III but hardly any sign of an incipient deceleration of the present rapid rate of growth.

That the population increase characteristic of modern times began in the 17th century or earlier is in accord with the usual macroscopic estimates. Both Willcox and Carr-Saunders took 1650 as their starting date and posited a subsequent continuous growth both of the world's population and, more specifically, of Europe's.<sup>11</sup> In a historical analysis of one particular country's population, however, the probability that the increase in numbers began so early requires reexamination of the usual thesis that its cause was wholly, or almost wholly, the decline in mortality.

<sup>11</sup> Both sets of figures are given in United Nations, *The Determinants and Consequences of Population Trends*, New York, 1953, p. 11.

#### MORTALITY

One can trace the course of Holland's mortality by fairly reliable statistics only since the middle of the 19th century. Attempts have been made to devise estimates from burial records of earlier centuries, but the data are so poor that they cannot yield even satisfactory local rates. In a recent article, De Haas has compiled from a number of contemporary sources the expectation of life at various ages from 1825 to date.<sup>12</sup> In Amsterdam, expectation of life at birth rose

TABLE 4. CRUDE DEATH RATES IN THE NETHERLANDS, 1850-1957

	Decennial Average	Range of Annual Rates from Low to High	
		National	Provincial
1850-59	25.5	22.3-31.0	17.7-40.4
1860-69	24.9	22.9-28.7	17.7-39.8
1870-79	24.4	22.2-28.4	18.0-39.0
1880-89	21.3	19.7-23.6	16.5-26.4
1890-99	18.7	16.9-21.0	13.8-18.9
1900-09	15.6	13.7-17.9	12.6-16.6
1910-19	13.5	12.3-17.5	11.2-20.0
1920-29	10.6	9.6-12.3	8.9-14.8
1930-39	8.8	8.4-9.6	6.8-11.0
1940-49	9.9	8.1-15.3	7.0-17.6
1950-57	7.55	7.3-7.8	6.6-9.0*

\* This is the range of what might be termed the normal death rate. In the new province of the Northeast Polder, with an almost total absence of elderly people, the death rate was only 1.7 in 1951. In Zeeland in 1953, after the main dikes broke and several of the large islands were flooded, the death rate rose to 11.4.

slowly from about 35 years in 1825 to about 38 years in 1845. For the whole of the country, this index was about 38 years in 1845 and only 40 years in 1875; but from that date on the rise has been much faster and, apart from World War II, without interruption.

As Table 4 shows, the remarkable decline in the Dutch death rate over the past century has been, more precisely, only since

<sup>12</sup> H. K. de Haas, "De bevolkingsgrooten gedurende de laatste eeuw," *Nederlandsch Tijdschrift voor Geneeskunde*, 94 (July 8, 1950), pp. 1972-1977. For the period 1825-1845, the calculation is based on Lobatto's study of the population of Amsterdam, which undoubtedly differed somewhat from the rest of the country in its mortality.



around 1880.<sup>13</sup> The 30 years preceding that date saw little change, either in the decennial averages or in the extremes of the considerable variation from year to year and from one province to another. The same trend can be noted in infant and child mortality, which in this period constituted a large fraction of the general death rate.<sup>14</sup> The reasons for the decline from the relatively high plateau on which the death rate rested in the middle of the last century are, of course, no mystery. Both specific cures for various diseases and highly significant improvements in the environment began to be developed with accelerating speed in the last decades of the 19th century. That is to say, the application of the most efficient means of death control effected not a transition from a static to a growing population, but the quickening of the rate of growth from 0.75 to 1.25 per cent per year that, as we have noted, also took place in the 1870s.

Was the considerable population growth before the introduction of modern medicine and public health also the consequence, either wholly or mainly, of a prior decline in mortality? And, if so, how can we account for this fall in the death rate? While it is not possible to answer these questions directly from mortality statistics, at least plausible hypotheses can be suggested from known institutional changes and their probable effect on the death rate.

The conclusions that McKeown and Brown reached in their important paper on mortality in 18th century England are relevant also to other European countries of that period.<sup>15</sup> They divided the possible causes of a reduction in mortality into three broad classes, as follows:

<sup>13</sup> The figures are calculated from the convenient compilation in A. Polman, *Ontwikkeling en huidige stand van de sterfte in Nederland en België*, The Hague: Vereniging voor Demografie, 1951.

<sup>14</sup> Infant mortality fluctuated around an almost constant mean from 1840 to 1880, and the age-specific rates for children and adolescents began to fall only in the 1870s. For a good discussion illustrated by a striking graph, see J. H. de Haas, "Van strijd tegen sterfte naar strijd voor gezondheid," *Wetenschap en Samenleving*, 13 (May, 1959), pp. 59-63.

<sup>15</sup> Thomas McKeown and R. G. Brown, "Medical Evidence Related to English Population Changes in the Eighteenth Century," *Population Studies*, 9 (November, 1955), pp. 119-141.

(1) Specific preventive or curative therapy. In the Netherlands as in England, most treatments of the various important causes of death can be discounted for the period earlier than the middle of the 19th century. It is a moot question whether fever hospitals, for example, helped restrict contagion by the semi-quarantine they imposed or raised the death rate by the fact that virtually all persons who entered them would be infected.<sup>16</sup> So long as bleeding was the first treatment for illness, the contribution that physicians made to their patients' health was minimal; so long as something like half of surgical patients died of infection, it can be questioned whether surgeons saved more patients than they killed. "It might safely be said," McKeown and Brown conclude, "that specific medical treatment had no useful effects at all, were it not for some doubt about the results of the use of mercury in syphilis, iron in anaemia, cinchona in malaria, and inoculation against smallpox."

(2) A change in the balance between the virulence of the infective organism and the resistance of the host. In specific instances—for example, the transformation of scarlet fever from a frequently fatal disease to a relatively trivial complaint—this was probably the decisive factor. The general effect of such changes on the long-term trend in the death rate, however, was probably slight.

(3) Improvements in the environment. By the partial elimination of the other two classes, this would seem to be the major cause of any important decline in mortality before about 1850. It is difficult to analyze these improvements, not only because data of all kinds are less numerous and less accurate before that date, and because the relation between environmental changes and presumably consequent declines in mortality are typically vague, but because it is hardly possible to speak of "improvements" in Dutch living conditions during the century from 1750 to 1850.

It was during this period, a hundred years

<sup>16</sup> Indeed, the hospital that Herman Boerhave (1668-1738) established in Leiden set a new standard for cleanliness and care of patients, but however important it was as a training center, the fact that it had fewer than two dozen beds tells how little effect it can have had on the conquest of the mortality of that time.

of almost unrelieved economic depression, that the first systematic studies were made relating mortality in the Netherlands to the environment. As early as 1770, the Academy of Sciences was sufficiently interested in this relation to offer a prize for the best answer to the question, "What human diseases derive from this country's physical conditions?" and the competition stimulated a larger number of persons to statistical research.<sup>17</sup> They and their counterparts in other countries laid a necessary base for the rapid advances in understanding during the past century.

There is very little in these early statistical studies to suggest a rise in the standard of living. Take the matter of food supply, one of the more important environmental influences on mortality. The Netherlands of the 17th century was ahead of the rest of Western Europe in its agricultural techniques. In the 18th century, thus, the first stimulus to the transformation of the English countryside—improved drainage and fertilizers, new crops, better breeding of farm animals—was an imitation of Holland. There can be little doubt that in both 17th century Holland and 18th century England the better and more varied diet of the populace resulted in better health. But for the latter decades of the 18th century and the first half of the 19th, the Dutch data recount mostly inadequacy, often misery. In the 1840s, when the potato blight spreading across Europe invaded the Dutch fields, the endemic deficiency developed into a near-famine. "Food consumption, at least in the cities, was just as low as in Ireland."<sup>18</sup>

The variation in infant mortality can also be explained in part by diet. In some regions and among the upper classes generally, babies

were breast-fed. But where mothers had to work, they fed their infants on bread soaked in water with a bit of milk or even gin. When the babies cried, they were given a piece of rag in which a piece of chewed bread with sugar had been tied. "This murderous thing," as De Vooys terms it, went by a variety of local names, but everywhere it was more infectious than nutritious.

Living conditions of the poor, particularly in the cities, were deplorable. Often a family of eight shared one bed. Almost one-tenth of the population of Amsterdam lived in damp cellars. According to various urban samples, infant mortality ranged from one-third to one-half. The correlation between size of township and the rate of infant mortality was positive until the 1880s; with the more rapid improvement of urban health facilities the correlation was reversed during the following twenty years.<sup>19</sup>

One reason for the high rates of urban mortality, both infant and general, is that many Dutch cities are in Holland Province, most of which is below sea level. This fact was certainly relevant to their state of health before the full development of modern engineering. The average death rates for 1841–1860 ranged from above 32 per thousand population in the low-lying townships of Holland Province and Zeeland, to below 22 per thousand in the high-lying townships in the East. The segregation of sewage from drinking water was especially difficult in the western provinces, and there were recurrent outbreaks of cholera until the 1860s. What was termed "swamp fever" (*moeraskoorts*) was actually a group of diseases, which each year ran through a seasonal cycle—influenza and malaria in the spring; in June and July diarrhea among infants, often linked to typhus or bacterial dysentery, whose incidence increased in the fall; and at the end of the year the various respiratory diseases. The drinking water in Zeeland was particularly poisonous: on one occasion in the 1780s, of 1,040 Swiss troops stationed in Sluis, only 12 or 13 could stand on their feet after just one month.<sup>20</sup>

<sup>17</sup> See Van Nierop, "De aanvang," *op. cit.* An interesting commentary on these works is given in two articles by A. C. de Vooys: "De opkomst van de medische geografie in Nederland," *Geografisch Tijdschrift*, 4 (1951), pp. 1–8; "Een regionale statistiek uit het begin der 19e eeuw," *ibid.*, 1 (1948), pp. 110–114.

<sup>18</sup> I. J. Brugmans, *De arbeidende klasse in Nederland in de 19e eeuw (1813–1879)*, The Hague: Nijhoff, 1925, p. 155. See also A. C. de Vooys, "De sterfte in Nederland in het midden der 19e eeuw: Een demogeografische studie," *Tijdschrift van het Koninklijk Nederlandsch Aardrijkskundig Genootschap*, 68 (1951), pp. 233–271; P. Geyl, *Geschiedenis van de Nederlandse stam (1751–1798)*, Amsterdam: Wereld-Bibliotheek, 1959, 3, pp. 59–61 and *passim*.

<sup>19</sup> Centraal Bureau voor de Statistiek, *Sterfte van kinderen beneden het jaar in elke gemeente van Nederland*, The Hague, 1910.

<sup>20</sup> Callenfels, as cited in De Vooys, "De sterfte in Nederland," *op. cit.*



In short, there is good circumstantial, though not decisive statistical, evidence to support the thesis that, at least for some social classes and regions, the death rate rose from the average of, say, 1650–1750 to that of 1750–1850. So long as public-health measures were relatively primitive, the congestion of the cities increased the danger of contagion; and under such circumstances the growth of cities would tend to increase mortality. There was probably also a decline in the living standards and especially the diet of the mass of the people. Extant accounts of 17th century food habits are concerned principally with the well stocked tables of the bourgeoisie, but in this relatively prosperous period even the poor probably ate better than their more numerous counterparts in the 1820s and 1830s, certainly better than in the 1840s.

If there was any decline in general mortality, then, it was probably quite small. Was it great enough to account for the increase in population—taking only the period measured by national censuses—from roughly 2.1 million in 1795 to almost 3.1 million in 1850? Or is there not a *prima facie* case here for the probability that fertility rose?

#### FERTILITY

In the conventional model of the demographic transition, it is assumed that Stages I and II were characterized by a more or less constant fertility at close to the physiological maximum. The population growth during Stage II—the consequence thus, wholly or almost wholly, of the fall in mortality—pushed parents to adopt the small-family system, which was based on a new rationalist attitude toward conception and the various contraceptive means invented or popularized during the 19th century.

While this model has a certain rough validity, there is little evidence on the face of it to support some of the details. Reproduction up to the physiological maximum is not the typical practice among either primitive peoples or preindustrial civilizations. Conscious family limitation did not have to wait for mechanical and chemical contraceptives; it can be effected by coitus interruptus, abortion, or infanticide—methods as old as

human history.<sup>21</sup> The average size of the family, moreover, depends not merely on the parents' will but on the variety of cultural, religious, and magical norms governing the age at marriage, the proportion of adults that marry, the remarriage of widows, the frequency of marital intercourse, and the like.

The conscious regulation of family size in late medieval and early modern Europe was in part accomplished by coitus interruptus, in part by abortion and infanticide.<sup>22</sup> A more significant check to fertility, however, had been gradually inculcated: the principle that a man might not marry until his living was assured.<sup>23</sup> In some cases, this norm was spelled out in detailed regulations of particular institutions. In other cases, it was strong enough to govern family formation without being specified in written laws. The principal

<sup>21</sup> Compare A. M. Carr-Saunders, *The Population Problem: A Study in Human Evolution*, Oxford: Clarendon Press, 1922. The thesis of this interesting work, that the conscious restriction of fertility is characteristic of all cultures, is documented in a long bibliographical appendix in which references to ethnological works are classified under R (prolonged restriction of intercourse), A (abortion), and I (infanticide).

<sup>22</sup> The opposition of the Catholic Church to these latter practices was vehement and specific enough to suggest that they were common. Five means of controlling family size were specifically forbidden—inducing sterility by drugs or incantations, aborting the fetus by violent exercise, killing the infant at birth, refusing to nurse one's child, and accidentally sleeping on it. See J. C. Russell, *British Medieval Population*, Albuquerque: University of New Mexico Press, 1948, p. 160. As late as the 17th century, when Vincent de Paul established the charitable order associated with his name, one impetus to his act was to furnish founding hospitals as a functional substitute for the continuing high rate of infanticide.

<sup>23</sup> This process in England is suggested by the etymology of the two words, *husband* and *anlepiman*. The word *husband* derives from two words meaning "house" and "dwell," and its original meaning (still preserved in *husbandman* and *husbandry*) was a householder, a man who had a home. The Middle English word for a single man was *anlepiman* ("only man"). These two terms, one referring to property and the other to marital status, gradually became associated as opposites, *anlepiman* coming to mean a man who had no living; and therefore could not marry, and *husband* a man who was able to care for a family and therefore could get (or, eventually, was) married. See George F. Homans, *English Villagers of the Thirteenth Century*, Cambridge: Harvard University Press, 1941, Chapter 10.

check to unlimited procreation in the Dutch countryside of several centuries ago, the joint household, is a good example of the second type.

In the Netherlands as in all Germanic countries, the sib remained an important legal body until the late Middle Ages.<sup>24</sup> And in many parts of the Dutch countryside, the extended family functions still today as a meaningful social organization. Until rather recently a discussion of it could have been based on nothing more than the impressionist writings of folklorists, plus a few incidental jottings by social scientists; but since the war the three-generation household has suddenly become a "social problem," to be studied by social workers, churches, and government agencies. This new interest has culminated in Kooy's excellent sociological analysis,<sup>25</sup> based in part on a questionnaire survey of the Achterhoek (literally, "back corner"), an agrarian region in the province of Gelderland. But this study is also relevant to other areas where a strong organization of the extended family still persists, and to a historical analysis of the Dutch countryside as a whole.

In its typical form, the joint household can be described as follows.<sup>26</sup> One of the sons (or where there are no sons, one of the daughters) is designated as the sole heir to the family farm, either explicitly in a legal document or implicitly by the tradition that all accept. When he marries, his bride comes to live under his parents' roof. In principle, the heir's brothers and sisters leave the farm; in practice, they often remain, unmarried uncles and aunts with a status between that of family members and servants. Variation in the present-day expression of this tradition is illustrated in Table 5.<sup>27</sup> The normal household consists of two families of successive generations (lines 1 and 2, plus some

TABLE 5. PERCENTAGE DISTRIBUTION IN PATTERNS OF JOINT RESIDENCE IN TWO REGIONS OF THE ACHTERHOEK

	Graafschap	Lijmers
1. Two families of successive generations	49.9	38.6
2. Family with one grandparent	27.5	27.0
3. Family with an unmarried uncle or aunt	8.6	18.2
4. Two families of the same generation	0.1	0.4
5. Family with a more distant relative	4.2	7.4
6. All other patterns	9.7	8.4
	100.0 (3,918)	100.0 (740)

other families, probably, in which both grandparents had died). Attached to this nucleus, however, there may be an unmarried sibling of the heir or his wife (line 3), or a more distant relative (line 5), or servant or farm-worker, also unmarried (line 6). Note how seldom the property is shared by two families of the same generation (line 4). The restriction that this system imposes on fertility is patent. The main desideratum, that from generation to generation the farm remain undivided in the same family, is safeguarded, but to this principle is sacrificed the normal family life of a considerable proportion of the adult population.

Because of their frustration, this pattern has been inherently unstable under modern conditions. Whenever a change in circumstances makes it possible, the unmarried hangers-on of these joint households rush to set up their own homes and establish their own families. Thus, several times in Holland's recent history there has been an explosive rise in the fertility of certain areas or certain social classes:

(a) The extension of arable land by reclamation has had the paradoxical effect of aggravating population pressure. For the settlers on the polders being built out of the former Zuider Zee are mostly younger sons of farmers, many of whom in their prior status would have been unable to marry. And in the new settlements, in part because of the preponderance of young adults, the birth rate has on occasion been more than 70 per thousand population!<sup>28</sup>

<sup>28</sup> This situation is discussed at greater length

<sup>24</sup> See, e.g., G. A. Kooy, *Het veranderend gezin in Nederland: Een sociaal-historische studie*, Leerdam: Ter Haar & Schuijt, 1957, Chapter 3 and especially p. 41.

<sup>25</sup> G. A. Kooy, *De oude samenleving op het nieuwe platteland: Een studie over de familiehuishouding in de agrarische Achterhoek*, Assen: Van Gorcum, 1959.

<sup>26</sup> *Ibid.*, pp. 35-36.

<sup>27</sup> Calculated from Table VI, *ibid.*, p. 33. This pattern was influenced in this case by a severe housing shortage, the consequence in part of war damage.

(b) During the last quarter of the 19th century artificial fertilizers were introduced in the sandy regions of the East and South, and the greatly improved productivity of the soil made it possible to divide up family farms into viable units of smaller acreage. For two or three generations, it was possible in this way for a much larger proportion of young adults to marry and procreate. And today these regions generally have the highest fertility rates in the country, for it has been difficult both to reestablish the traditional pattern of family limitation by the nonmarriage of some adults, and to overcome the opposition of the various churches to family limitation by the use of contraceptives.<sup>29</sup>

(c) The joint household and the limitation on human fertility that it implies disappeared earlier where agriculture was based on the naturally more fertile clay soil. This process has been analyzed in detail by Hofstee, particularly for the Oldambt, a region in northeast Groningen. Until the 18th century, the farm laborers there lived almost as members of the farmer's family, sleeping in the same house, eating at the same table, working together during the day, talking over common interests in the evening. From about 1775 on, this patriarchal relation began to disappear, to be supplanted eventually by a sharp class differentiation. The well-to-do landowners underwent an *embourgeoisement* that transformed them from traditional peasants into modern farmers. Even earlier than in the cities, they adopted a small-family system by which the relation between the land and the number of landowners was kept almost constant. The farm workers, converted into a landless proletariat, were released from the institutional and moral inhibitions

to procreation implicit in the old system. In the century following 1775, their number in the province of Groningen increased four times.<sup>30</sup>

(d) If the fertility of agricultural workers increased when they became a *rural* proletariat, should this not have taken place also when they moved to the towns and were there released from the same checks to procreation? That urban fertility in the Netherlands was higher than that in the countryside until about 75 years ago has long been an established fact, but its implications have seldom been explored until a recent paper by Hofstee.<sup>31</sup> In order to supplement the existent compilations of township data, Hofstee com-

<sup>30</sup> See E. W. Hofstee, *Het Oldambt: Een sociografie*, Groningen: Wolters, 1937, pp. 193-235. Of the several articles in which the theme of this work is analyzed more intensively, the most recent is "De ontwikkeling van de huwelijksvruchtbaarheid in het Oldambt in de periode 1880-1950," in J. Brummelkamp et al., editors, *De wereld der mensen*, Groningen: Wolters, 1955, pp. 295-353. This is a report of the marital fertility of the total sample of first marriages in three townships of the Oldambt, with a detailed analysis of differentiation by social class and religion. Among the well-to-do farmers, the completed fertility fell from below four children in the last decades of the 19th century to about 2.5 in marriages contracted around 1910, and it has remained at approximately this level. Among agricultural workers, completed family size began at approximately six children around 1880, then fell with varying speeds according to the religious denomination. Among the small local businessmen, the trend in family size has been less clearly defined, but in general this group stands intermediate between farmers and agricultural workers.

<sup>31</sup> E. W. Hofstee, "Regionale verscheidenheid in de ontwikkeling van het aantal geboorten in Nederland in de 2e helft van de 19e eeuw," *Koninklijke Nederlandse Akademie van Wetenschappen, Akademiesdag*, 7 (1954), pp. 59-106. A typical instance of a certain blindness usual in earlier analyses can be seen in a paper by the highly competent demographer and former director of the Central Bureau of Statistics, H. W. Methorst. In an article published in 1913 he compared the trend in birth rates in townships with more than and fewer than 20,000 inhabitants—a rough but sufficiently accurate differentiation between "urban" and "rural." According to his data, the urban birth rate was higher until 1890, the two were almost identical at the end of the 1890s, and only in the 20th century was the urban birth rate lower. Yet his analysis is limited to a discussion of the very latest trend. See H. W. Methorst, "Nederlandsche bevolkingsstatistiek," *Economist*, 62 (1913), pp. 126-154, 250-259, 367-400.

in Petersen, *op. cit.*, pp. 103-108. See also Sjoerd Groenman, "L'assèchement du Zuiderzee et le problème de la population aux Pays Bas," *Population*, 7 (October-December, 1952), pp. 661-674; "Zuiderzee gronden en sanering van de kleine boerenbedrijven," *Landbouwkundig Tijdschrift*, 64 (January, 1952), pp. 5-14.

<sup>29</sup> This relation between soil type and human fertility patterns has been analyzed by E. W. Hofstee in "De landbouw en de migratie," *Economisch-Statistische Berichten*, 35 (December 20, 1950), pp. 1024-1026; "De functie van de internationale migratie," *Tijdschrift voor Economische en Sociale Geografie*, 15 (January-February, 1949), pp. 10-22.

piled from provincial and township archives his own breakdown for 1850-1880, and thus obtained a valuable new base for analyzing fertility during this transitional period. Average birth rates for 1851-1855 showed a regional patterning almost precisely the opposite of that to be seen today. The highest birth rates at that time—35 per thousand or more—were in the agricultural provinces with a clay soil (Zeeland, Friesland, and Groningen) and the country's urban center (North and South Holland). Birth rates in specific cities varied somewhat, but in general they were close to the level of the surrounding countryside. This differentiation is not due, as one might suspect, to a difference in age structure; it holds also when the fertility is compared by other measures.

At one time the three-generation household was standard in the Dutch countryside, and something like it seems to have existed also in the cities.<sup>32</sup> In both cases, the principal check to fertility was by the relatively high proportion of the population that remained single. It is reasonable to suppose that the same forces that prevented the marriage of some tended to postpone that of the others. However, the secular trend in the median age at marriage cannot be realistically discussed in statistical terms, for the following reasons:

(1) Data are completely lacking for the earlier period.<sup>33</sup>

<sup>32</sup> So long as apprenticeship entailed living in the master craftsman's home-workshop, which was passed on from father to one son, the similarities with the system in the countryside were clear, though the number of persons affected was of course smaller. Some of the heterogeneity supposedly typical of urban life can be discerned in Dutch cities, but until well into the modern period the guilds—or at least the style of economic organization that they represented—remained one important factor. They were in decline in the 18th century, but toward its end there were still 51 trade and craft guilds in Amsterdam, for example. Guilds were formally abolished during the French occupation, but remnants of the system persisted into the 19th century. See Cornelius Wiskerke, *De afschaffing der gilden in Nederland*, Amsterdam: Paris, 1938; A. J. M. Brouwer Ancher, *De gilden*, The Hague: Loman & Funke, 1895.

<sup>33</sup> The degree to which this is the case can be illustrated by an article by van Nierop that, with painstaking effort, has winnowed every bit of information from the marriage records in Amsterdam for the last decades of the 16th century. As is generally the case until national compilations of civil records began in the middle of the last cen-

(2) As is well known, in societies where the postponement of marriage constitutes an important method of family limitation, the age generally rises and falls according to economic conditions.<sup>34</sup> Given the poor statistics, it is therefore still more difficult to discern a possible long-term trend underlying these fluctuations.

(3) In any case, the trend in the median age at marriage of the whole population, if it were possible to establish it for the early modern period, would not reveal the changes presumably taking place in several of the social classes.

Even so, although the point cannot be statistically documented, it is reasonable to assume that the same institutional changes that permitted a larger number to marry also tended to reduce the age at marriage.<sup>35</sup>

With respect to fertility, a more important consequence of the breakdown of the moral and institutional norms inherent in the joint household was the probable rise in illegitimacy. Like all Germanic countries, the

tury, direct data on age at marriage were scarce and deficient in the Amsterdam records, and a number of complicating factors made it difficult even to estimate the trend. See Leonie van Nierop, "De brudegoms van Amsterdam van 1578 tot 1601," *Tijdschrift voor Geschiedenis*, 48 (1933), pp. 337-359; 49 (1934), pp. 136-160; 52 (1937), pp. 144-162.

<sup>34</sup> This was true of the Netherlands until about 1870, although apparently less so than of some other countries. See J. H. van Zanten and T. van den Brink, *Population Phenomena in Amsterdam in Comparison with Other Big Towns* (Statistical Communication No. 103a), Amsterdam: Municipal Bureau of Statistics, 1939, pp. 4-39.

<sup>35</sup> In contrast to rural Ireland, there is apparently no impetus to the early marriage of the heir. In Ireland the young man takes over the management of the farm when he gets married, but in the Netherlands one of the frequent sources of friction noted in recent publications is that, on the contrary, even responsible family men are still given no voice in running the property. In some areas—for example, the bulb-growing region in Holland Province—fathers try to keep all their sons single as long as possible, paying them small wages for long hours of work. See, e.g., I. Gadourek, *A Dutch Community: Social and Cultural Structure and Process in a Bulb-Growing Region in the Netherlands*, (Netherlands Institute of Preventive Medicine, Publication XXX), Leiden: Stenfert Kroese, 1956, pp. 173-174. The effect of this practice on fertility is ambivalent, however, for while marriage is postponed, each horticulturist has an economic incentive to have many sons, which reinforces his traditional, often Catholic, morality.



Netherlands has inherited a tradition of "window wooing."<sup>36</sup> By this folk norm, premarital intercourse is usual, and marriage does not take place until the bride is pregnant. According to a government survey made just before World War II, the percentage of forced marriages in the Netherlands ranged from just over 13 in large towns to 16 in villages.<sup>37</sup> In a number of areas, generally quite fundamentalist in religion, the custom is still more prevalent.<sup>38</sup> It is something of a misnomer, however, to call these "forced marriages," a term that suggests a more or less random liaison. With respect to both the timing and the mate chosen, these are usually planned, or at least half-planned, conceptions; and so long as the village's social control is unbroken, marriage follows them almost inevitably. Yet this is a system that all but invites dalliance once the control is released—and in the 19th century the urban illegitimacy rates were generally high.<sup>39</sup>

#### CONCLUSIONS

##### In the theory of the demographic transi-

<sup>36</sup> This is the literal translation of "venster vrijen," one of the terms by which the custom is designated in Dutch. Most books on the family written in English pass lightly over this important element of the West European cultural tradition. The best general account is by a Swede: K. R. V. Wikman, *Die Einleitung der Ehe: Eine vergleichend ethno-soziologische Untersuchung über die Vorstufe der Ehe in den Sitten des schwedischen Volkstums*, (Acta Academiae Aboensis, Humaniora XI.1), Abo: Abo Akademi, 1937.

<sup>37</sup> Kooy, *Het veranderend gezin . . .*, op. cit., p. 146.

<sup>38</sup> In the village of Staphorst, for example, of the 87 first births in 1937-1938, 34 were within seven months of the marriage ceremony. In the 1920s, a number of ministers cooperated in a determined effort to stamp out the practice and fulminated from their pulpits against the young people who spent Saturday night in sin and then came to church on Sunday morning. Finally they succeeded—but only in having the traditional night for "window wooing" changed to Friday. See Sjoerd Groenman, *Staphorst: Sociografie van een gesloten gemeenschap*, Meppel: Stenvert, 1948 [?], pp. 96, 153ff.

<sup>39</sup> In Amsterdam, for example, almost one out of every five births was illegitimate for the period 1811-1824, the earliest for which this information is available, and the percentage remained high until the last quarter of the century. See "Statistiek der bevolking van Amsterdam tot 1921," *Mededeelingen van het Bureau van Statistiek der Gemeente Amsterdam*, 67, 1923.

tion, the population growth of an area undergoing modernization is divided into three stages: (1) a more or less static population at high levels of fertility and mortality; (2) a period of constant fertility and falling mortality, with a consequent rapid increase in population; (3) a more or less static population at more efficient levels of birth and death control. It is generally believed that the population of the Western world has increased continuously from 1650 on, at the latest. The decline in mortality that is used in the model to explain this growth cannot be documented for anything like so long a period. In the Netherlands, as generally in the Western world, the most dramatic rise in life expectation dates from the last quarter of the 19th century, and for the prior several hundred years the presumed fall in mortality can neither be proved from the statistics nor even—for a substantial portion of this period—plausibly related to institutional changes.

The unlikelyhood of a decline in mortality is increased when we examine more closely the other half of the balance—the assumption that fertility remained more or less constant at a high level until it began to fall with the advent of the modern small-family system. It is strange that this thesis has not been challenged more often. It is not even in accord with the established statistical record of the 19th century. Because of the accident that the high point in the British birth rate coincided with the Bradlaugh-Besant trial, most demographers know that in that country there had been an upward trend prior to that date. But something of the same pattern can be seen in the course of the fertility of most other West European countries. The French pattern of a steady decline in natality since the beginning of reliable records, which has often been taken as the model with which to analyze the fertility of the Western world, seems rather to be an exception.<sup>40</sup>

For the early modern period a statistical analysis must be based on data poor enough to make it suspect, but the hypothesis that

<sup>40</sup> See, e.g., Gerhard Mackenroth, *Bevölkerungslehre: Theorie, Soziologie und Statistik der Bevölkerung*, Berlin: Springer, 1953, pp. 122-134. This section is divided into two parts, the first on the general development of fertility in Northwest and Central Europe, the second on the reasons for the exceptional development in France.

there was a rise in fertility is strongly reinforced by what we know of the institutional changes that accompanied modernization. The Middle Ages bequeathed to the present-day Western world a social system with built-in guards against excessive procreation. Whatever their form, these were expressions of the principle that a man might not marry and beget offspring until he had established an appropriate place for himself which would enable him to carry out his family responsibilities. Perhaps the most precise form of this type of institutional check was contained in the regulations of the English guilds, which prohibited marriage during a long apprenticeship and made it difficult for a period thereafter.<sup>41</sup> In the Netherlands (apparently as in most other Continental countries), the control by the guilds was less rigid, but there too one function of the apprenticeship system was to prescribe, or at least to facilitate, this norm of responsible parenthood.

At any time prior to the most recent past, however, the vast majority of all populations lived in the country, and for a long-term analysis the rural institutions governing fertility demand the most attention. In the Netherlands this institution was the joint household.<sup>42</sup> In principle, in each generation

only one person on each farm married and had children. The household also furnished a function and a home, however, for the unmarried. Whether as uncles or aunts to the farm-owner's children, or as more distant relatives, or as servants and farm-workers, these had a place as meaningful parts of an economic and social unit. The limitation to fertility in the joint household was efficient, but it was dependent on the maintenance of the institutional forms. As the joint household began to disintegrate, in part because in modern times the nuclear family has been more strongly emphasized, or because the unmarried hangers-on found an opportunity to escape from what they began to perceive as sexual and social frustration, it was inevitable that fertility should rise. And this rise can be demonstrated in a number of particular instances.

Generalizing from the Dutch case, we can posit the following hypothesis in place of the present theory concerning fertility trends in a Western country undergoing modernization.<sup>43</sup> In the *traditional family* typical of the preindustrial period, the postponement of marriage, plus the nonmarriage of a portion of the population, constituted an onerous but efficient means of holding fertility in check. In the *proletarian family*, typical of the mass of either rural or urban workers released from the prior institutional and normative restrictions, there was no effective bar either to early marriage or to procreation. Indeed, social control was often barely strong enough to compel marriage once a child had been conceived. In the *rational family* type, which arose first among the middle classes during the 19th century and then gradually spread to the rest of society, a sense of parental responsibility reappeared, and with it a limitation of family size. The average age at marriage rose again, and later the same end was achieved with less privation by the use of contraceptives. Thus, in order to trace the changes in the fertility of any country, we would need statistical data on completed

<sup>41</sup> "Apprenticeship in its fully grown Elizabethan form requires that those learning any trade then practiced in England should serve an apprenticeship for seven years or until he was twenty-four years of age, with the possible exception of agriculture in which it was sufficient that he should attain the age of twenty-one if the parties had been unable to agree on twenty-four. It is clear that these provisions were looked upon quite as much as a check on the exuberance of youth as essential for the technical education of the country." G. Talbot Griffith, *Population Problems of the Age of Malthus*, Cambridge: Cambridge University Press, 1926, p. 112.

<sup>42</sup> In their excellent analysis of the interrelation among social structure, family type, and fertility, Davis and Blake argue, on the contrary, that a joint household favors high fertility. This is indeed the case in classical China and India, the examples they use to illustrate their thesis. In such a household, marriage and procreation are feasible as soon as they are physiologically possible, for the supervision of household affairs does not depend on the social maturity of each individual couple. See Kingsley Davis and Judith Blake, "Social Structure and Fertility: An Analytical Framework," *Economic Development and Cultural Change*, 4 (April, 1956), pp. 211-235. Krause is presently analyzing

the relation between the joint household and fertility in 18th century England.

<sup>43</sup> The following discussion derives largely from Hofstee, "Regionale verscheidenheid," *op. cit.* Hofstee acknowledges a debt to Mackenroth (*op. cit.*, p. 474), who in turn notes that a germ of the hypothesis is to be found in Malthus.



family size by social class from the 17th century at latest. By this time the disintegration of institutional checks to fertility, the development of new means of death control, and the resultant increase in population were all under way. These data will never become

available. But such statistical records as we do have, at least in the Dutch case, support the thesis that the population growth characteristic of the modern West must be explained as the consequence of both a rise in fertility and fall in mortality.

## MIGRATION AS A FUNCTION OF POPULATION AND DISTANCE \*

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*Changes in spatial distribution of population are partly determined by the existing distribution of population and by the deterrent effect of distance. It is tentatively concluded that the number of migrants declines more than proportionately with increasing distance. The hypothesis of allometric growth, in contrast to the P/D hypothesis, implies that the attractive power of cities increases more than in proportion to their population. Historical development of various city hierarchies does not support the hypothesis of allometric growth. It appears to be a special case, characterizing structural shifts from subsistence to commercial agriculture, and from agriculture to industry.*

IF the distribution of population in space is known at any point in time, it is possible theoretically to predict population distributions at later points in time, or to reconstruct distributions at earlier dates, provided that the functional relations between migration and its main determinants are also known. Total migration from or to a city is directly related to its population. The distribution of migrants from a city by place of destination, and of migrants to a city by place of origin, is inversely related to distance of migration. These two quantitative relations, if given specific values, are sufficient to determine the spatial distribution of migrants from a single city by place of destination, and to a single city by place of origin. Additional information is required, however, to determine the absolute number of migrants and to establish the spatial distribution of all migrants without reference to specific cities of origin or of destination.

Net migration is not simply related to population and distance. The direction of net migration is a question of relative levels of employment, wages, and living standards, as well as various non-economic factors. (In particular cases other factors may be signif-

icant, for instance, the lure of climate and legend for migrants to California and Florida.) The total volume of migration is also in large part determined by economic levels: both absolute levels of economic activity; and the extent of differences in wage and employment levels, cost of living, and the like. The same economic index therefore will provide information both on the quantity of migration and on the direction of net migration.

The scope of the present discussion is limited to the first two determinants of migration, population<sup>1</sup> and distance, admittedly because they are easily measured. They are not simple in their effects. Population is an index of variety of employment opportunities, cultural facilities, social contacts, and the likelihood of resident relatives and friends. Distance subtends the effect of costs, intervening opportunities, information, and social separation, all of which are functions of distance.

For the third determinant, economic levels, we lack a single index: employment levels, wage levels, and cost of living indexes are all

\* The writer is indebted to T. R. Anderson of Yale University for valuable criticisms and suggestions.

<sup>1</sup> Census population of administratively defined cities except that when separate figures are given for a central city and its suburbs, their sum is employed, this total being more relevant for inter-city migration.

serious contenders. And none of them is simple since migration is a consequence of the differences in economic levels between areas as well as of the absolute economic standing of each area. Communications networks and amount of information are also of critical importance and largely independent of economic levels.

Mechanical models of migration as a function of population and distance imply certain growth patterns in city hierarchies. We propose to discuss these implications for city-size distribution, and to check them against historical trends. In particular, here the allometric growth hypothesis is checked against both migration models and historical data. This hypothesis regards the growth rate of a city relative to the growth rate of (urban) population as an increasing function of its rank in the system of cities.

#### THE INFLUENCE OF DISTANCE

One of the leading theoretical formulations of the spatial pattern of migration, Zipf's P/D hypothesis,<sup>2</sup> postulates an inversely proportional relation between the rate of total migration between two places and the distance of migration. A number of studies using U.S. census data have tested the validity of this hypothesis. Anderson, after surveying and evaluating the literature, concludes that distance should be raised to a power greater than one and less than two, and expresses the opinion that the exponent should be a variable rather than a constant. He also concludes that distance should be given greater weight and population less weight.<sup>3</sup>

<sup>2</sup> George K. Zipf, "The P1P2/D Hypothesis: On the Intercity Movement of Persons," *American Sociological Review*, 11 (December, 1946), pp. 677-686; Zipf, *Human Behavior and the Principle of Least Effort*, Cambridge: Addison-Wesley, 1949, pp. 386 ff. Zipf's hypothesis, as well as some, but not all, attempts to verify it, have been conceived in terms of movement, or travel, rather than migration properly speaking. Furthermore, they are concerned with total movement of persons between any two cities, rather than net migration. See also J. Q. Stewart, "A Measure of the Influence of a Population at a Distance," *Sociometry*, 5 (February, 1952), pp. 62-71.

<sup>3</sup> Theodore R. Anderson, "Intermetropolitan Migration: A Comparison of the Hypotheses of Zipf and Stouffer," *American Sociological Review*, 20 (June, 1955), pp. 287-291. Anderson lists most of the relevant publications on the subject.

A second major approach to an empirical law of migration, Stouffer's, substitutes for distance the concept of intervening opportunities, defined in practice as the cumulated number of migrants from all points residing between the point of origin and the point of destination. Stouffer's hypothesis states that the number of migrants between two points is inversely proportional to the number of intervening opportunities and directly proportional to the number of opportunities at the point of destination. Opportunities in the city of destination are defined as the total number of migrants from all points residing there.<sup>4</sup> The measurement of opportunities in terms of earlier migration involves us in an infinite regress: the number of migrants and their distribution over space is dependent on the number of previous migrants and their distribution over space *ad infinitum*. If opportunities are proportional to population, and if population is uniformly (although discretely) distributed in space, this formulation reduces to Zipf's hypothesis. Tests of Stouffer's hypothesis for the United States and Sweden have been described as "encouraging" but have not been clearly confirming.

Empirical studies on the relation between migration and distance in England and Wales strongly indicate that the number of migrants from a given area declines more than proportionately with distance of migration. The relation between migration and distance was examined for workers moving from Northumberland and Durham, and from Glamorgan and Monmouthshire, to all other counties in England and Wales.<sup>5</sup> The elasticity<sup>6</sup> of labor migration with respect to

<sup>4</sup> Samuel A. Stouffer, "Intervening Opportunities: A Theory Relating Mobility and Distance," *American Sociological Review*, 5 (December, 1940), pp. 845-867.

<sup>5</sup> Helen Makower, Jacob Marschak, and H. W. Robinson, "Studies in the Mobility of Labour," *Oxford Economic Papers*, 1 (October, 1938), pp. 100 ff., esp. p. 106 and p. 118; *ibid.*, 4 (September, 1940), p. 44.

<sup>6</sup> The concept of elasticity, which is derived from economic theory, defines a ratio of percentage changes in two variables, in this case the ratio:

$$\frac{\text{percentage change in number of migrants}}{\text{percentage change in distance of migration}}$$
 Values greater than 1 indicate that the number of migrants from any point (or to any point) changes more than proportionately with the distance of migration. Since migration is an inverse distance function, the above ratio has a negative sign.

distance proved to range between 1.6 and 2.1 for Oxford; it was somewhat lower in the other two cases—1.46 for labor migration from Northumberland and Durham, and 1.21 from Glamorgan and Monmouthshire.

It is reasonable to expect distance to be less of an inhibiting factor to migration in the New World than in the Old.<sup>7</sup> The fact that the P/D hypothesis is not clearly disconfirmed by the data used to test it suggests that the elasticity of migration with respect to distance in the United States is not far from unity. It should be pointed out that the British data refer to labor migration whereas American studies refer to all migrants. The effect of distance on migration of workers varies with the number of dependents. There is some indirect evidence for Anderson's view that the exponent of the distance is a variable within the United States. Reilly, after discussing the extraordinarily large trade areas of some Texas cities, tentatively attributes them in part to a different "psychology of distance."<sup>8</sup>

#### THE INFLUENCE OF CITY SIZE

*Allometric Growth.*—Various writers have described the historical development of a system of cities as an example of allometric growth.<sup>9</sup> In allometric growth the rate of growth of a city relative to the growth of

urban (or total) population is a variable, directly related to the city's rank in the urban hierarchy, a rank measured by its population; the bigger the city, the higher its relative rate of growth.<sup>10</sup> We need not consider the countervailing forces which modify and eventually stop this differential growth. We here assume the fact of allometric growth in some stage of development of a city hierarchy, and inquire about its implications for migration theory.

Cities may grow through natural increase or migration. (A third process, annexation, is not considered; it is neither a universal nor a continuing cause of population change, and its operation is contingent upon an administrative definition of city boundaries.) A differential birth rate or a differential death rate or a combination of the two may account for allometric growth even in the absence of migration. Such an explanation may apply, although exceptionally, to some very underdeveloped countries where birth rates are unaffected by city size but where death rates, as a result of differential availability of medical care, drugs, and public health facilities and measures, are inversely related to the size of cities. In the general case, however, differentials in rate of natural increase by size of city, far from explaining allometric growth, work counter to it, since birth rates tend to be lower in large cities than in small towns, whereas death rates do not. The study of the historical development of city-size distributions therefore provides a rough overview of the relation between size of city and net migration.

If differences in rates of growth of cities of various sizes are to be largely accounted for in most cases by migration, then the attractive power of a city to migrants must increase more than proportionately with its size; the "P" in the P/D hypothesis must have an exponent greater than unity. Zipf's hypothesis, which requires that rate of

<sup>7</sup> E. W. Bakke et al., *Labor Mobility and Economic Opportunity*, New York: Wiley, 1954, p. 113.

<sup>8</sup> William J. Reilly, *The Law of Retail Gravitation*, New York: Pilsbury, 1953, p. 16.

<sup>9</sup> "In general, as a community's P size increases, it is proportionately ever more likely to receive immigrants and ever less likely to lose emigrants with the result that the rate of growth in the P population sizes of the larger communities is likely to be a constant p power of their respective P sizes. . . . With communities of smaller P population sizes, the very opposite is likely to be the case . . . the rate of net decrease will be ever larger as P is ever smaller." Zipf, . . . *the Principle of Least Effort*, op. cit., pp. 358-359. And on page 367: ". . . the ever larger communities will grow ever more at the expense of the number and sizes of the smaller communities." See also George K. Zipf, *National Unity and Disunity*, Bloomington: Principia Press, 1941, pp. 127 ff., 141; J. Q. Stewart, "Empirical Mathematical Rules Concerning the Distribution and Equilibrium of Population," *Geographical Review*, 37 (July, 1947), p. 467; Martin J. Beckmann, "City Hierarchies and the Distribution of City Size," *Economic Development and Cultural Change*, 6 (April, 1958), pp. 243-248.

<sup>10</sup> The formula for allometric growth,  $Y = bX^k$ , as originally applied by Huxley, denotes the relative growth rates of an organism (Y) and of a part of that organism (X). When  $k = 1$ , growth rates are the same. As applied to city growth, k is a function of city rank in the system of cities comprised in Y, such that the average of k's for all cities (X's), weighted by population, is equal to unity, and such that  $k > 1$  for larger (higher ranking) cities and  $k < 1$  for smaller (lower ranking) cities.

growth be invariant by city size, therefore is incompatible with allometric growth. The compatibility of Stouffer's hypothesis hinges on the relation between population and opportunities. Only if opportunities increase more than proportionately with population is it compatible; but since the uniqueness of Stouffer's hypothesis consists in substituting intervening opportunities for population and distance, the stipulation of a determinate relation between opportunities and both population and distance (this is necessary if population is uniformly distributed over space) destroys it: Stouffer's approach is not commensurable with allometric growth.

A more precise confrontation of the two incompatible hypotheses, allometric growth and the P/D migration hypothesis, requires knowledge of the distribution of migrants from a given place for a given distance by size of city of destination. For completeness it is desirable also to check the rates of migration by size of city of origin, since allometric growth is the result of net migration differentials, which in turn may be the consequence of out-migration differentials by size of city as well as in-migration differentials. We must determine whether allometric growth is the result of migration from the countryside alone, or also the effect of out-migration differentials among cities (with rates inversely related to city size). In the former case, allometric growth ceases once the shift from country to city is completed; in the latter, it continues but at a slower rate.

Two studies of the relation between migration and distance which did not allow for differential migration rates by size of city of destination note that migration to the largest cities in the areas studied—Memphis, Tennessee, in one case and Stockholm, Sweden, in the other—distort the spatial pattern of migration.<sup>11</sup> In both cases migration to the largest city was much greater than predicted either by the P/D hypothesis or by Stouffer's intervening opportunities hypothesis. Before accepting Anderson's suggestion that the exponent of D should be greater than one, we

should instead test the possibility that the exponent of P should be greater than one, as required by the hypothesis of allometric growth. With our limited empirical knowledge we cannot rule out the possibility that migration is a city rank function rather than a function of absolute population. Migration rates may vary with relative city size within an economic region, rather than with absolute city size. Thus the migration rate might be as high to Memphis as to Stockholm, and as high to Stockholm as to Tokyo, despite the large differences in absolute size.

Since larger cities are more widely spaced than smaller cities, the distance from any one point to a city is nearly always a direct function of the size of that city, however measured. Therefore, in many cases, as equally good fits may be obtained by adjusting the exponent of D as by adjusting the exponent of P. If D is adjusted, however, the exponent must be less than one to account for allometric growth, whereas empirical data previously mentioned imply an exponent greater than one. It is concluded, therefore, that the logically correct explanation of allometric growth is not a distance but a population function.

No writer on the subject has placed restrictions on the form of allometric growth, other than that a city's relative rate of growth is an increasing function of its rank. It is in part determined by the rate of natural increase, here assumed to be invariant by city size. In particular, no verdict has been passed as to whether rate of growth differentials are functions of absolute differences in city size or relative differences, or both. Some views on this point may be inferred from a study of city size-rank distributions for various countries and various periods of time given by Zipf. These distributions when plotted on double log paper approximate straight lines, according to Zipf. The inappropriateness of some of the samples used has been discussed elsewhere, as has the straight-lines assumption.<sup>12</sup> What is relevant here is the slope of the lines: is the slope for the same country or area on different dates the same, or is there a tendency for growth of city populations to be accompanied by a

<sup>11</sup> John Folger, "Some Aspects of Migration in the Tennessee Valley," *American Sociological Review*, 18 (June, 1953), pp. 253-260; Eleanor C. Isbell, "Internal Migration in Sweden and Intervening Opportunities," *American Sociological Review*, 9 (December, 1944), pp. 627-639.

<sup>12</sup> Charles T. Stewart, Jr., "The Size and Spacing of Cities," *Geographical Review*, 48 (April, 1958), p. 231.



steeper slope? Is the slope the same for different countries even though cities of the same rank are of very different populations? An affirmative answer to the latter question is implied by Zipf and J. Q. Stewart, suggesting that relative size or rank, not absolute size, is the relevant determinant of differential growth rates.

*Historical Trends in City Hierarchies.*—Historical trends in city size-distribution, if they are to serve as checks on the P/D or the allometric growth hypothesis, imply rank consistency over time. If the slope of a distribution hides marked reordering of cities over time, it is no indication of the applicability of either the P/D or (as usually stated) allometric growth hypotheses, both of which refer to specific cities. It is still possible to discuss the concept of allometric growth in terms of slope alone, as a function of rank order, but without reference to specific communities, that is, without the assumption of ranking consistency. The P/D hypothesis, however, breaks down altogether.

There is nothing in the theory of an urban hierarchy which would lead to an expectation of marked inconsistency over time. Nor does theory suggest any plausible explanation. Quantitative growth alone may lead to development of new, higher-order functional classes, and to the merger of two or more lower classes or to elimination of the lowest class. But such ranking inconsistency as results from growth of some cities in a class to form another, higher class, or shrinkage of some townlets whose functions have been absorbed by larger towns, is of very limited range. So is the inconsistency resulting from changes in census procedures and definitions.

The large ranking inconsistencies which result from development of new resources, shifts in political or settlement boundaries, major technological developments, mass migrations, and the like, fall outside the explanatory bounds of the theory of an urban hierarchy. Nor can these extraneous influences be accounted for either in their origination or in their effects by any mechanical function of existing population distribution and distance. Migration models therefore cannot explain the existing distribution of population. They prove circular in nature, with each population distribution

being a function of an earlier one, and a determinant of a later distribution.

Should we then discard the P/D hypothesis as an approximation to urban growth? Not at all. However imperfect, population and distance remain as significant indices of a city's attractive power. They remain the main explanation of growth in a stable, not to say static, system of cities. Where the system is unbounded in area, or technologically dynamic, or otherwise qualitatively changing, population and distance, although still operational, may be overridden by more powerful factors.

Madden's study of the growth of U.S. cities from 1790 to 1950 shows frequent and quite large changes in city rank.<sup>13</sup> Ranking of cities on a regional instead of a national basis may reduce somewhat the fluctuations in ranking but not sufficiently to make the data compatible with a P/D or allometric growth explanation of city growth.

The results obtained in predicting city populations by the "ratio method"<sup>14</sup> have also been incompatible with either hypothesis. This method bases its predictions mainly on a constant relation between the growth of the city and the predicted growth of the region or nation, that is, on an allometric growth assumption. The ratio method reveals a large margin of error for individual cities even in short-range predictions of ten to twenty years, and fails entirely for cities in rapidly growing regions, such as the Pacific Coast.

There is no doubt that some countries exhibit allometric growth of the city size-distribution; Zipf makes much of the case of Germany, not a particularly spectacular

<sup>13</sup> Carl H. Madden, "On Some Indications of Stability in the Growth of Cities in the United States," *Economic Development and Cultural Change*, 4 (April, 1956), pp. 236-252; Madden, "Some Spatial Aspects of Urban Growth in the United States," *ibid.*, 6 (January, 1958), pp. 143-169.

<sup>14</sup> See the following articles by Robert C. Schmitt and Albert H. Crosetti: "Accuracy of the Ratio Method for Forecasting City Population," *Land Economics*, 27 (November, 1951), pp. 346-348; "Short-Cut Methods of Forecasting City Population," *The Journal of Marketing*, 17 (April, 1953), pp. 417-424; "A Method of Estimating the Population of Cities," *American Journal of Public Health*, 44 (November, 1954), pp. 1426-1427.

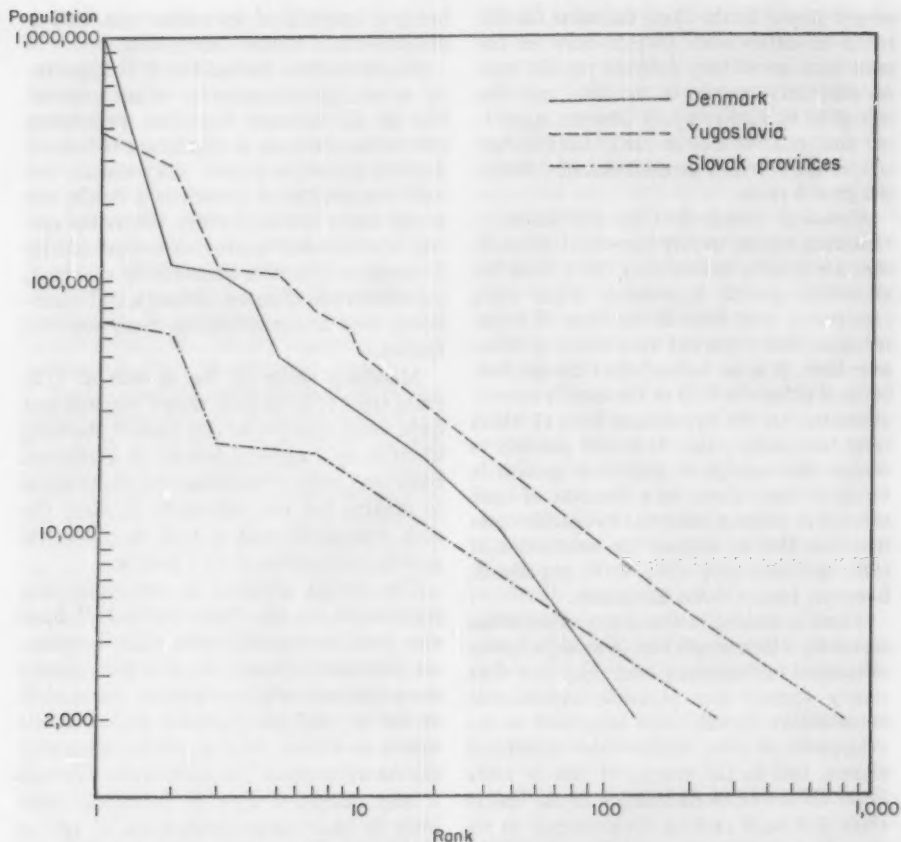


FIGURE 1. A CONTRAST IN ECONOMIC DEVELOPMENT: THE SIZE-RANK DISTRIBUTION OF TOWNS IN DENMARK (1950), YUGOSLAVIA (1948), AND THE SLOVAK PROVINCES (1930)

example.<sup>15</sup> Japan and Denmark clearly show an increasing slope over time of city distribution ordered by size.<sup>16</sup> Other examples could be adduced. In no case was ranking consistency considered. But it is also possible to find historical examples revealing proportional rather than allometric growth, the United States providing the outstanding illustration,<sup>17</sup> if one accepts with Zipf the view that a region as large as this is suitable for verification of the rank-size rule for

cities. There are countries widely different in their urban populations and in the sizes of their large cities which have approximately the same slope, as well as countries with similar total urban populations and sizes of largest cities, for example, India and the United States,<sup>18</sup> with significantly different slopes.

*Economic Structure, Progress, and City Growth Patterns.*—A more careful scrutiny of both examples and exceptions reveals two possible generalizations. Underdeveloped countries seem to have in every case exhibited a slope with low value, well under one; advanced countries have in every case a steeper slope, generally with a value around

<sup>15</sup> Zipf, *National Unity and Disunity*, *op. cit.*, p. 141.

<sup>16</sup> On Japan and India, see J. Q. Stewart, "Empirical Mathematical Rules . . .," *op. cit.*, p. 467; on Denmark, see C. T. Stewart, Jr., *op. cit.*, p. 231.

<sup>17</sup> Zipf, *National Unity and Disunity*, *op. cit.*, p. 44.

<sup>18</sup> *Ibid.*, p. 137.

one. Figure 1 contrasts an advanced country, Denmark, with two relatively backward areas, Yugoslavia and Slovakia. All three are agricultural.

A second and separate distinction can be made between mainly agricultural and mainly industrial countries or regions, as measured by the composition of their external trade.<sup>19</sup> The agricultural region, apparently regardless of stage of development beyond the point needed to unify the region in question, is characterized by a dominant central city many times the size of any

other, and conversely by the small size of the second-rank cities. Manufacturing countries, on the other hand, have several or even many leading cities in about the same size-class, usually with no central city several times the size of the second largest city.

Figure 2 illustrates the differences between agricultural and industrial countries ranging in population from less than three to nine million. All of them are small in area. Although the Australian state of Victoria is larger than any of the countries, it is so strongly focused on its capital, Melbourne, that the problem of multi-regionalism does not arise.

It is instructive to contrast, first, the two highly industrialized countries, Belgium and Switzerland, with all the rest. The former have a substantial number of sizeable cities; the others generally have only one such city, the capital. It is also desirable to contrast countries of approximately the same population, namely, Belgium and Portugal, both with about nine million, although Belgium has a somewhat larger urban population; and Switzerland with five and Denmark with about 4.5 million and about the same urban population. Ireland and Victoria, with nearly three million persons each can also be contrasted with Switzerland.

Ideally, for purposes of measurement we should select countries with about the same urban population and the same per capita income, but with widely different industrial structures. This ideal cannot be met: the countries cited above with approximately the same urban populations differ substantially in per capita income. Income differences are in turn reflected in occupational and industrial structure. Although Portugal, Ireland, Victoria, and Denmark are grouped as mainly agricultural economies, in fact only 23 per cent of the labor force in the high-income country of Denmark (in 1950) and 11.2 per cent in Victoria (in 1954) are employed in agriculture; whereas the corresponding figure for Ireland is 39 per cent and for Portugal 49 per cent (1957 estimates). A much larger percentage of employment in Victoria and Denmark is accounted for by consumer services. The component of industry which least reflects the differences in per capita income, which most nearly approximates an autonomous measure of differences in industrial

<sup>19</sup> There are many possible ways of measuring economic structure. A common and useful approach, the distribution of the labor force by economic sector, is not quite appropriate for the present purpose because it reflects to a considerable extent the standard of living of the area measured, its productivity in major sectors of its economy, and therefore differences in the structure of demand. Only for countries at the same level in all these respects—income, productivity, and standard of living—would differences in labor force distribution by sectors correspond to differences in economic structure.

The method used here is the balance-of-trade approach. If a country is a net exporter of agricultural products by a wide margin, then it is mainly agricultural. If it is a net exporter of manufactures, then it is industrial. This crude approach suffices for the countries used as examples. It would have to be refined considerably to indicate other than gross differences in economic structure. For instance, net exports of this or that category mean little if the country's foreign trade is inconsequential relative to its domestic trade and national income. Such a self-sufficient country—whether it has 80 per cent of its labor force in agriculture, as in the case of many underdeveloped countries, or 50 per cent (USSR), or 10 per cent (United States)—has a fairly balanced economic structure in terms of its demand pattern. If commodity trade is sadly out of balance with imports greatly exceeding exports (Ireland's imports are nearly twice its exports), then the country may be a net importer of all commodity classes. Its trade balances on agricultural, or industrial, goods alone may be useless as indices of economic structure; the entire trade must be examined to determine in which categories the country is most and least dependent on imports.

For countries whose demand pattern is determined mainly by the state, diverging markedly from the pattern which might otherwise prevail, international comparability *via* the balance of trade approach is not meaningful.

Victoria's trade balance with the other Australian states is not available. It should not, however, modify the conclusion that Victoria, like Australia as a whole, is a net exporter of agricultural products by a wide margin, and a net importer of manufactured goods.

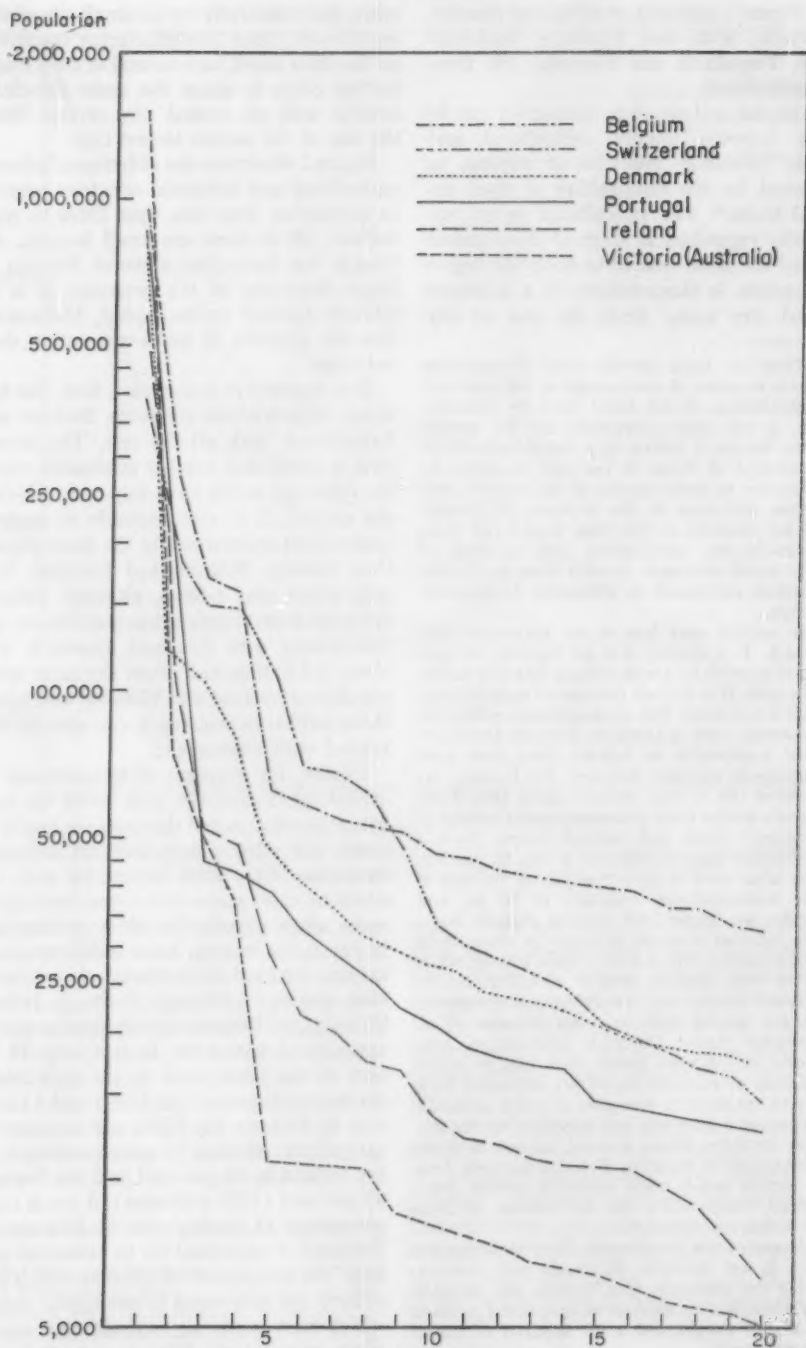


FIGURE 2. INDUSTRIALIZATION AND SIZE—DISTRIBUTION OF CITIES



structure (rather than a measure of per capita income-induced differences), consists of the capital goods industrial sector and business services associated with it. Such poor countries as Russia and Japan may have as great or greater concentration of employment in these industries as wealthy countries like Canada and the United States.

The four agricultural countries—Victoria, Ireland, Portugal, Denmark—differ widely in the percentage of the labor force engaged in agriculture, in agricultural productivity, and in standard of living. Nevertheless their urban size-distributions show considerable similarity, and clear dissimilarity from the urban size-distributions of the two industrial countries, Switzerland and Belgium.

Since the size-rank distributions typifying these two types of economic structure are substantially different, a major shift from agriculture to manufacturing (other than agricultural processing and agriculture-supporting industries) will undoubtedly result in differential growth rates, with the cities in intermediate size-ranges growing much faster than smaller cities and also faster than the leading city. If the leading city is omitted, the structural shift from agriculture to industry (involving also a spatial shift from rural to urban residence) will have the appearance of allometric growth, defined as growth rates increasing with city size. The largest city, however, does not conform to the hypothesis. Incidentally, the only example found of a decreased slope is that of Austria following World War I.<sup>20</sup> The decrease in slope from the prewar period was associated with the decline of Vienna as the center of an empire and with reduced hinterlands for Vienna and a few of the larger Austrian towns.

Although from a historical standpoint industrialization and urbanization have gone hand in hand, they are not identical processes, and the relation between their respective rates is a loose one. Since the form of the town-size distribution is affected by industrialization, one cannot expect a rigid relation between urbanization alone and relative town sizes, or between either of these and migration differentials by size of towns.

Switzerland, for example, although one of the most highly industrialized countries in the world, is among the least urbanized in Europe.<sup>21</sup> Australia, on the other hand, has long been highly urbanized (79 per cent urban in 1954), although industrialization is of recent origin and has not yet advanced very far by West European standards.

In regions and nations where land is the dominant source of livelihood and where subsistence agriculture prevails, self-sufficiency on a local basis is almost complete. In most respects relevant to human interaction there are no larger regions. Most towns, symbiotic with their local hinterlands, are isolated. The distribution of city sizes in such regions is subject to no general rule, being dependent on exogenous factors of a specific nature: soil fertility, climate and other natural determinants of crop yield, the relation between productive land area and agricultural population, crops, farming techniques, land tenure practices, and other cultural determinants of crop yield, not to speak of various additional factors affecting the number of people and their rate of growth. Therefore, it is not possible to state in general terms the pattern of growth in city sizes in an economic region increasing in population and productivity, and shifting from subsistence to commercial agriculture; this shift is accompanied by the development of a town network and a town hierarchy, in place of a congeries of isolated, unrelated towns. The lack of pattern logically precludes generalization, not merely lack of data. In such a pre-commercial region, the size of the capital city and its size relative to the provincial capitals, if any, depend largely on the extent of the region's political unification, the degree of centralization, and the socio-political structure.

As a country commercializes its agriculture, the capital city grows more slowly than the second tier of cities, for the multicellular character of a self-sufficient economy is replaced by nationwide or regionwide specialization and exchange, requiring the development of large regional market centers. Copenhagen, 17 and a half times as large as the

<sup>20</sup> Zipf, *National Unity and Disunity*, *op. cit.*, p. 194.

<sup>21</sup> Kurt B. Mayer, *The Population of Switzerland*, New York: Columbia University Press, 1952, p. 253. See also Dorothy S. Thomas, "Internal Migrations in Sweden," *American Journal of Sociology*, 42 (November, 1936), p. 350.

next Danish city in 1801, is now only six and a half times as large. In Sweden, Stockholm has undergone a relative decline since 1800, when it was six times the next largest city; for the last half century it has remained just twice as large as Göteborg. Exceptions arise when the country originally lacks political unity, and economic unification is accompanied by the growth of area-wide political organs domiciled in a capital. Another exception occurs when commercialization is based on some sort of plantation system and is geared to foreign markets; the capital city may then grow as a major port. The gain from commercialization, accruing mainly to a small landowner class, will be spent in the capital and abroad, whereas if the gain were widely distributed in the first instance, its expenditure would stimulate the growth of regional market towns with many complementary amenities.

#### CONCLUSIONS

Limited empirical data do not support the hypothesis that migration is proportional to the population of the city of destination, nor that it is inversely proportional to the distance of migration. There is strong presump-

tion that the distance exponent should be greater than one. The evidence on the influence of population is more complex and less clear-cut. The hypothesis of allometric growth requires that the exponent of  $P$  be greater than one. But the historical development of city size-distributions does not support the hypothesis of allometric growth except during a major structural shift from agriculture to industry (and even then, only for city hierarchies, not for individual cities), and possibly from subsistence to commercial agriculture. Even in these cases the largest city in a region proves to be an exception. The relation between city population and migration rates can only be specified in a broader context: the equilibrium distribution of cities by size for a given stage of economic development and of economic structure. A study of the historical development of many city hierarchies is needed both as a rough measure of migration rate differentials by size of city and of growth-rate differentials of cities by size or by rank. From the results of such a study it should be possible to define more precisely the equilibrium distribution of urban population by size of city, for each of several stages of economic development, and for each of several ideal-types of economic structure.

### URBAN STRUCTURE AND INDUSTRIALIZATION \*

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*Previous studies have established the existence of a high positive association between the extent of urbanization, as measured by proportion of total population in administratively defined cities, and industrial development. The concentration of urban population, as reflected in the proportion of urban population in metropolitan areas, fails to show as close a relationship to industrial development. It is hypothesized that metropolitanization is strongly influenced by the structure of the industrialization process, and not only by industrialization as such. As a test of the hypothesis, Japanese industrial organization and employment patterns are analyzed in terms of their influences upon metropolitan structure.*

THE usefulness of the metropolitan area concept in the analysis of the urbanization process has long been recognized. An increasing number of official and private agencies are engaged in the reporting and

analysis of demographic data for urban units larger than the administratively defined city. The adoption of these units is the result of a recognition of the fact that the arbitrary political boundaries of cities often break up population concentrations which function-

\*The materials upon which this paper is based were collected and processed while the author was a research associate with International Urban Re-

search, University of California, Berkeley, during the academic year 1957-58.

ally are a single urban complex. Unfortunately, the criteria applied for delimiting metropolitan areas show significant variations from country to country, as well as from census to census for the same country. The opportunities for international comparative studies in metropolitanization and its consequences therefore have been limited.

The International Urban Research project at the University of California, Berkeley, has made an initial step toward providing the necessary materials for international metropolitan analysis. A recent publication from this project reports the application of a standardized set of criteria for delimitations and provides world-wide coverage of current metropolitan population data.<sup>1</sup> The present paper reports an exploration, based upon the International Urban Research data, of the relationship between metropolitanization and industrial development. Specifically, the hypothesis to be tested is that metropolitanization is positively associated with level of industrialization. That is, the growth of functionally urban population, both inside and contiguous to administratively defined cities, is correlated with the expansion of industrial rather than agrarian activities.

#### METROPOLITANIZATION AND ECONOMIC DEVELOPMENT

Previous studies have established the existence of a high positive association between urbanization and economic development. In computing levels of urbanization, these studies have utilized officially reported populations in all administratively defined cities or segments thereof. For instance, a correlation between percentage of economically active males in nonextractive employment for all countries of over one million total population (*circa* 1945) has an index of correlation ( $r$ ) of  $+0.885$ . Limited data indicate that this relationship also holds within the histories of specific nations: United States (1820-1940),  $+0.997$ ; England and Wales (1841-1931),  $+0.985$ ; France (1866-1946),  $+0.970$ ; and Sweden (1870-1940),  $+0.967$ .<sup>2</sup>

<sup>1</sup> International Urban Research, *The World's Metropolitan Areas*, Berkeley: The University of California Press, 1959.

<sup>2</sup> Kingsley Davis, "World Regions and the Correlates of Urbanism," paper read at the annual

The availability of metropolitan data with relatively complete world coverage makes possible further investigations of the structure of urban population in its association with economic development. The proportion of population residing in politically defined cities provides an index of the *extent* of a nation's urbanization; the proportion of population residing in metropolitan areas highlights the *depth* or concentration of that urbanization.<sup>3</sup> Here the essential question is whether or not both of these facets of the urban process bear the same relationship to industrialization. A simple ratio of the metropolitan population to the total urban population gives a rough index of metropolitan concentration.<sup>4</sup> This index, in turn, can be correlated with nonextractive employment as a practical test of the association of metropolitanization and industrial activity.

Table 1 shows the 49 nations for which data on urbanization, metropolitanization, and economic activity are available *circa* 1950. The index of metropolitan concentration is shown in Column 3. A linear correlation of this index and the percentage of males in nonextractive employment results in an index of correlation ( $r$ ) of  $+0.776$  for the total sample. Metropolitanization therefore bears a relatively close association to industrialization, but not so close as that between the extent of urbanization, as measured by population in cities of 100,000 and over, and industrial activity.

Of primary significance is the fact that the association of metropolitanization and eco-

meeting of the American Sociological Society, September, 1952.

<sup>3</sup> For comparison of the relationship of conventional administrative definitions of urban population and metropolitan definitions, see Jack P. Gibbs and Kingsley Davis, "Conventional Versus Metropolitan Data in the International Study of Urbanization," *American Sociological Review*, 23 (October, 1958), pp. 504-514.

<sup>4</sup> This index is usually less than one; however, it is possible for it to exceed unity. Population which according to census definition is nonurban but which meets given criteria is counted as metropolitan. In a highly metropolitan but small country, such as Switzerland, the administrative definition of urban population has lagged behind the development of concentrations of functionally urban populations in areas contiguous to cities. The result is that Switzerland's index of metropolitanization (1.13) reflects a greater metropolitan population than total urban population as defined in her census.

TABLE 1. INDICES OF URBANIZATION, METROPOLITANIZATION, AND ECONOMIC DEVELOPMENT FOR SELECTED COUNTRIES, circa 1950

Country	Date	Per Cent Total Population Defined Urban <sup>a</sup> (1)	Per Cent Total Population in Metro- politan Areas <sup>b</sup> (2)	Metropolitan Index: Column (2)	Per Cent Males in Nonagricultural Employment <sup>c</sup> (4)
				Column (1) (3)	
50% or More Urban					
United Kingdom	1951	80.3 <sup>d</sup>	77.0	0.959	93.6
Israel	1949	71.3 <sup>d</sup>	55.9	0.784	88.9
Germany	1950	71.0 <sup>d</sup>	46.7	0.658	82.2
Australia	1947	68.9	55.4	0.804	80.9
Denmark	1950	67.3	45.5	0.676	71.0
United States	1950	64.0 <sup>d</sup>	55.9	0.873	91.5
Belgium	1947	62.7	41.4	0.660	86.4
Argentina	1947	62.5	43.8	0.700	70.3
Canada	1951	61.6	45.5	0.739	76.5
New Zealand	1951	61.3	33.6	0.711	77.8
Cuba	1953	57.0 <sup>d</sup>	26.0	0.456	53.1
France	1954	55.9	34.4	0.615	73.8
Netherlands	1947	54.6	45.6	0.835	80.0
Venezuela	1950	53.8	26.2	0.487	52.3
Japan	1950	53.5	36.9	0.716	59.8
25-50% Urban					
Austria	1951	49.2	38.9	0.791	75.1
Sweden	1950	47.5	31.8	0.669	74.7
Mexico	1950	42.6 <sup>d</sup>	20.3	0.477	42.2
Union S. Africa	1951	42.6	31.5	0.739	53.0
Ireland	1951	41.5	23.4	0.564	54.0
Italy	1951	40.9 <sup>d</sup>	27.3	0.667	57.5
Spain	1950	37.0	24.2	0.654	46.6
Greece	1951	36.8	22.0	0.598	50.5
Switzerland	1950	36.5	41.2	1.129	78.5
El Salvador	1950	36.5	11.9	0.326	26.7
Colombia	1951	36.3	18.6	0.512	36.8 <sup>d</sup>
Brazil	1950	36.2	17.6	0.486	37.3
Peru	1940	36.1	10.4	0.288	33.7 <sup>d</sup>
Panama	1950	36.0	23.9	0.664	40.8
Nicaragua	1950	34.9	13.3	0.381	23.1
Paraguay	1950	34.6	15.6	0.451	35.1 <sup>d</sup>
Iraq	1947	33.8	17.5	0.518	45.0 <sup>d</sup>
Costa Rica	1950	33.5	19.9	0.594	37.4
Finland	1950	32.3	17.0	0.526	53.9
Norway	1950	32.2	21.8	0.677	68.6
Portugal	1950	31.2	19.6	0.628	47.9
Honduras	1950	31.0	7.3	0.235	16.9
Egypt	1947	30.1	19.6	0.651	47.6
Ecuador	1950	28.5	14.9	0.523	38.1
Malaya	1947	26.5	12.7	0.470	39.2
25% or Less Urban					
Guatemala	1950	25.0	10.6	0.424	24.0 <sup>d</sup>
Philippines	1948	24.1	10.3	0.427	36.4
Dominican Rep.	1950	23.8	11.2	0.471	34.7
Turkey	1950	21.9	14.0	0.639	30.6
India	1951	17.3	7.8	0.451	30.6
Ceylon	1946	15.4	9.5	0.617	49.4
Haiti	1950	12.2	6.0	0.492	13.4
Pakistan	1951	11.4	5.1	0.447	23.7
Thailand	1947	9.9 <sup>d</sup>	6.8	0.687	18.3

<sup>a</sup> Unless otherwise designated the urban population is that reported in the *Demographic Yearbook, 1955*, Tables 7 and 8.

<sup>b</sup> Metropolitan area delimitations prepared by International Urban Research, University of California, Berkeley.

<sup>c</sup> Unless otherwise designated the source for these figures is the *Demographic Yearbook, 1956*, Table 12.

<sup>d</sup> From official census reports and yearbooks.



nomic development becomes closer as the extent of urbanization increases. This is to say that as the proportion of population in administratively defined cities increases, the concentration of urban population in metropolitan areas reflects more clearly the level of industrial development. The 15 countries with more than 50 per cent of their total population defined as urban have a coefficient of correlation of  $+0.841$  between metropolitan concentration and industrial activity, as shown in Table 2. This association decreases to  $+0.828$  for the 41 countries with more than 25 per cent population defined as urban. Both these associations are closer than the  $+0.776$  cited above for the total sample of 49 nations.

These results confirm the linkage of metropolitanization and economic development, but the association is a qualified one. High levels of urbanization and industrial activity are positively associated with metropolitanization, but the development of population agglomerations which are metropolitan in character is not invariably linked to these two traits. Metropolitan growth is characteristic of some countries of relatively low urbanization and industrial development. Table 3 shows that the average number of metropolitan areas of over one million population for countries of less than 25 per cent urban is almost twice that for countries between 25 per cent and 50 per cent urban. India, for example, although low in total level of urban residence (17.3 per cent) and in male non-extractive employment (30.6 per cent), contains some of the world's largest metropolitan

TABLE 2. CORRELATIONS OF METROPOLITANIZATION INDEX AND ECONOMIC DEVELOPMENT FOR SELECTED COUNTRIES, *circa* 1950

Percentage Urban <sup>a</sup>	Number of Countries	Coefficient of Correlation (r) <sup>b</sup>
Over 50%	15	$+0.841$
Over 25%	41	$+0.828$
Total	49	$+0.776$

<sup>a</sup> Per cent of total population residing in administratively defined cities.

<sup>b</sup> Linear correlation computed for percentage of economically active males in nonextractive employment and index of metropolitanization:

Per cent of total population in metropolitan areas

Per cent total population urban

Sources: See Table 1.

TABLE 3. DISTRIBUTION OF METROPOLITAN AREAS OF ONE MILLION POPULATION OR MORE BY LEVEL OF URBANIZATION FOR SELECTED COUNTRIES, *circa* 1950

Percentage Urban	Number of Countries	Number of M.A.'s One Million Plus	Average Number M.A.'s
Over 50%	15	44	2.9
25%-50%	25	14	0.6
Under 25%	9	9	1.0

Sources: See Table 1.

areas among her five urban agglomerations of over one million population.

#### THE BIASES OF MODERN METROPOLITAN THEORY

The greater portion of modern metropolitan theory is biased in two respects. The source data upon which knowledge of the metropolitan process is based represent a sample of nations confined largely to those which provide contexts of high standards of living and the technological implements of Western industrialism. This constitutes essentially a cultural bias in metropolitan analysis. The second bias is the tendency to view only the recent histories of metropolitan structures in these Westernized nations. This "unhistorical" quality hampers consideration of the early forms of the relationships of urbanization and metropolitanization, relationships which may have differed sharply from those currently observable.

The frequency with which private ownership of automobiles, increases in single family dwelling units, and long distance commuting patterns are cited in analyses of metropolitanization illustrates the cultural bias. Hawley's statement describing United States metropolitan growth in this respect is typical: "Following quickly upon reductions of the time and cost involved in local movements, which resulted from the introduction of motor vehicles and the hard-surfaced road, improvements in the transmission of electric power, the telephone, the radio, and more recently television, the urban community burst its narrow bounds and expanded over the surrounding country."<sup>5</sup> Investigations of

<sup>5</sup> Amos A. Hawley, *The Changing Shape of Metropolitan America*, Glencoe, Ill.: Free Press, 1956, p. 1.

United States metropolitan growth rarely omit noting this association of rapid expansion of metropolitan population and mass production of automobiles.<sup>6</sup> An expanding transport technology may well be crucial in the structure of recent United States metropolitanization, but its applicability in a general theory of metropolitan growth is doubtful. India, with five metropolitan areas of over one million population, produces only some 50,000 automobiles annually and has few automotive imports;<sup>7</sup> Japan in 1955 had only 7.8 automobiles per 1000 population but over one-third of her total population resided in metropolitan areas.<sup>8</sup>

Preliminary historical analyses in Western nations suggest that even where levels of urbanization and metropolitanization are both currently high, significant differentials existed during earlier periods. Manchester, England, a far from rare example, quite early in the industrialization process reached a size in excess of 100,000 population. Even before the coming of modern transportation facilities in the form of the steam railway, Manchester would appear to have reached metropolitan size and to have done so when the national level of urbanization in England was relatively low. This suggests the possibility that in the earlier phases of urban-industrialism in Western nations relatively low urbanization was coupled with relatively high metropolitanization. It is significant that even in Western nations such as England the appearance of metropolitan areas preceded development of the transportation and communication technology most frequently utilized

to explain metropolitan growth and structure.

It is clear that understanding the development of metropolitan concentrations historically in Western nations and currently in non-Western nations will require the search for factors other than technological or those associated with the technological expansion closely identified with Western industrialization. Genuine regularities in metropolitan structure and change *per se* will become evident only when these biases have been eliminated. The international data now available make possible an attempt at cross-cultural metropolitan analysis.

The deviant cases in the metropolitan-industrial relationship discussed above illustrate specifically opportunities for this approach. Certain regularities are evident in the deviations. In Latin American nations, for instance, comparatively higher levels of overall urbanization are associated with lower metropolitanization, irrespective of nonagricultural employment levels. Table 1 shows that Cuba and Venezuela have the lowest metropolitan indices of all nations with more than 50 per cent urban populations. At the other extreme, Guatemala has the lowest metropolitan index of all nations 25 per cent or less urban; while the lowest metropolitan index of the total sample is that for Honduras, though her total population is nearly one-third urban. In contrast, a group of Eastern and African nations which are low in economic development and extent of urbanization show comparatively high metropolitan levels. The following summary is illustrative of the major regional differences:

	Western Europe and Anglo-America	Latin America	The East and Africa
Industrialization	High	Moderate/Low	Medium/Low
Urbanization	High	High	Low
Metropolitanization	High	Low	High

<sup>6</sup> One of the more recent citations of this association appears in Leo F. Schnore and Gene B. Petersen, "Urban and Metropolitan Development in the United States and Canada," *The Annals of the American Academy of Political and Social Science*, 316 (March, 1958), pp. 60-68.

<sup>7</sup> 1958 *Hindustan Yearbook*, Calcutta: Sarkar, 1958, pp. 497-498.

<sup>8</sup> *Nihon toshi nenkan*, 1957 (Japan City Yearbook, 1957), Tokyo: 1957, pp. 426-427.

A detailed analysis of all the nations making up these categories is beyond the scope of this paper. However, an illustrative analysis of one non-Western nation, Japan, serves to confirm the hypothesis that the nature of industrialization, and not only industrialization as such, affects metropolitan structure significantly.

## JAPANESE INDUSTRIAL AND METROPOLITAN STRUCTURES

Japan has the lowest percentage of total population defined as urban and the third lowest percentage of males in nonagricultural employment of the 15 nations with more than half their total populations defined as urban (Table 1). Nevertheless the metropolitan index places her above many of the highly industrialized nations of the West. The contrast between Japan and Germany or France, for example, shows Japan to be less urbanized and less industrialized, but more metropolitan. Japan therefore illustrates again the lack of a direct association of urban-industrialism with metropolitan growth. Japan's industrial transition, with respect to both the nature of her economic organization and her population movements, was and continues to be of a kind that makes understandable her deviation from Western experience.

First, the paucity of cultivatable land within the home islands coupled with an industrial system which has never been able to reduce significantly the agricultural labor force, creates a constant pressure toward areal compactness of urban settlements. The agricultural density of Japan (males working in agriculture per square mile of cultivated land) is over 400, as against an average of 79 for Europe and ten for the United States and Canada.<sup>9</sup> Although nonagricultural employment and urban population show consistent increases throughout the last century, the absolute number of Japanese agricultural households has remained relatively stable: 5.6 million in 1872 and 6.1 million in 1951.<sup>10</sup> The competition from agricultural land usage and high rural densities mean that urban population increases are apt to take place within existing urban boundaries or in minimal annexations of contiguous territory. Therefore, Japan's rapid increases in urban population have tended to increase city sizes

to the metropolitan level rather than to create extensive suburbanization around the cities. It is perhaps overstating the contrast to cite the fact that 85.9 per cent of Japan's metropolitan population in 1950 resided in incorporated cities of 100,000 or more, in contrast to 55.9 per cent of the United States metropolitan population's residing in central cities,<sup>11</sup> although the direction of the contrast is valid.

In addition to these spatial pressures toward metropolitanization, Japanese economic organization tends to concentrate even further the urban population. Two basically dissimilar productive systems make up Japanese industrial structure. The so-called strategic industries—shipbuilding, chemicals, armaments, and the like—have been developed through governmental sponsorship and subsidy, and have since their beginning utilized the most advanced industrial technology. The development of this segment of the Japanese economy was created principally to implement Japan's early nationalistic political aims, rather than as a policy to improve directly standards of living for the population as a whole. Production of goods for home consumption has been largely left to household-handicraft industry. The "putting out" system is widely used in this type of production, even in the largest cities, and exists side-by-side with large-scale factories.<sup>12</sup> The result of this dual productive system is to increase greatly the proportions of the industrial labor force and the number of industrial establishments which fall into the "small" industry category. Of Japan's industrial establishments in 1954, 79.5 per cent had less than ten employees and these establishments employed 23.2 per cent of the industrial labor force. At the other extreme, only 1.2 per cent of Japan's industrial establishments had 100 or more employees, and these employed 38.5 per cent of those active in industry.<sup>13</sup>

With a limited number of large-scale industries, and these centered in a few major urban regions, the employment opportunities

<sup>9</sup> All figures circa 1940-1947. Kingsley Davis, "Population and the Further Spread of Industrial Society," *Proceedings of the American Philosophical Society*, 95 (February, 1951), pp. 11-12.

<sup>10</sup> T. Honda, "Historical Analysis of Population Problems in Japan," *Jinko mondai kenkyu* (Studies in Population Problems), 6 (June, 1950), p. 11. The Bank of Japan, *Economic Statistics of Japan*, Tokyo: 1951, p. 294.

<sup>11</sup> Hawley, *op. cit.*, p. 22.

<sup>12</sup> See especially E. B. Schumpeter, editor, *The Industrialization of Japan and Manchukuo*, New York: Macmillan, 1940.

<sup>13</sup> *Japan Statistical Yearbook, 1957*, Tokyo: 1957, Table 37.

TABLE 4. JAPANESE METROPOLITAN STRUCTURE, 1955

Composition of Metropolitan Area	Number	Total Metro-politan Population		Population in Incorporated Cities*		Population in Unincorporated Areas	
		Number (000's)	Per Cent	Number (000's)	Per Cent	Number (000's)	Per Cent
One incorporated city	32	5,907	16.7	5,907	18.1	—	—
Two or more incorporated cities	7	1,581	4.5	1,581	4.8	—	—
One incorporated city, plus unincorporated areas	12	3,272	9.2	2,833	8.8	440	15.8
Two or more incorporated cities, plus unincorporated areas	12	24,627	69.6	22,284	68.3	2,343	84.2
TOTAL	63	35,387	100.0	32,605	100.0	2,783	100.0

\* Cities incorporated as of the census of 1950.

Sources: *Population Census of 1950*, Vol. VII (Prefectural volumes).  
*1955 Population Census of Japan*, Vol. I.

associated with an expanding Japanese industrialization tend to channel urban population increases toward already existing urban areas. The industrial labor force engaged in subsistence production of the handicraft-household type functions in combination employment-residence locations within the cities or blends into rural areas where agriculture can become a secondary occupation. This fact helps to explain the significantly high proportion of agriculturalists within the administrative boundaries of even the largest of Japan's cities. Nerima ward, an outlying area of the city of Tokyo, for example, had 14.5 per cent of its economically active males in agriculture as late as 1950.<sup>14</sup>

Only the two largest of Japan's metropolitan areas have developed sufficiently to counteract the pressures toward concentration cited here. Tokyo-Yokohama and Osaka-Kobe show significant peripheral urbanization outside the administrative boundaries of the cities falling within these metropolitan areas. The remaining 61 of Japan's metropolitan areas emphasize the pattern of concentration within the cities. Table 4 summarizes the structure of Japanese metropolitan areas. In only 24 of Japan's 63 metropolitan areas

in 1955 did persons live outside the administrative boundaries of incorporated cities, and 84.2 per cent of this suburban population was contained in 12 of the 24 metropolitan areas. Moreover, 49 per cent of these 2.8 million suburbanites resided within either the Tokyo-Yokohama or Osaka-Kobe metropolitan area.

Japanese urban structure reflects not only city growth in response to industrial development, but a metropolitanization that is in large part the consequence of the specific social organization within which the Japanese industrial revolution occurred.

#### SUMMARY

This paper has explored important aspects of the relationship of metropolitan structure and economic development. Earlier studies of urbanization, as defined by the proportion of population residing in administratively defined cities, and of industrial development have shown a close association between the two. This is to say that the extent of a nation's urbanization parallels its industrial expansion. The concentration or depth of urbanization, as measured by the ratio of metropolitan population to administratively defined urban population, does not correlate as closely with economic development. It is suggested here that the source of this divergence lies in the fact that metropolitan

<sup>14</sup> *Population Census of 1950*, Vol. VII, Part 13, pp. 110-111.



growth is heavily influenced by the structure of the industrialization process, not by industrialization as such alone.

As a test of the hypothesis, the metropolitan structure of Japan was analyzed in terms of its relationship to industrial organization and employment patterns. The integration of traditional systems of household-based handicraft employment with large-scale industrial activities has retarded population developments outside city boundaries. Except in those metropolitan areas with significant concentrations of large-scale industries and extensive public transportation systems (Tokyo-Yokohama and Osaka-Kobe), Jap-

anese urban growth has meant an increase of population within central cities in contrast to the suburbanization or fringe growth experienced in most Western nations. Given these traits, Japan ranks relatively low among the industrial nations in commitment to non-agricultural employment and size of urban population, but relatively high in the metropolitanization of her urban structure. This type of analysis should help to develop theoretical forms of the metropolitanization-industrialization relationship, which in turn, should permit more precise formulation of projections of urban structure for nations currently undergoing industrialization.

## CHANGES IN THE RURAL-NONFARM POPULATION, 1930-1950 \*

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*The increasing concentration of the American population in larger urban places is well documented. It is hypothesized that such a concentration is also characteristic of the rural-nonfarm population and that this became more pronounced between 1930 and 1950. The hypothesis is supported by county census data for 1930, 1940, and 1950. During this period the rural-nonfarm population resided primarily adjacent to urban centers. This concentration increased until 1950 and probably 1960. In general, rates of increase were higher in counties containing larger places. The data suggest that a substantial proportion of statistically rural-nonfarm people are sociologically urban and lend support to changes in "urban" and "rural" definitions already made or under consideration by the Bureau of the Census.*

THE 1950 United States Census shows that there were over 6,000,000 rural-nonfarm dwellers who were living in that year in counties which contained no urban place. There were nearly 10,500,000 more who were residents of counties with no center of at least 10,000 persons but which did contain one or more urban places. But at the same time another 22,000,000 rural-nonfarm persons lived in counties which included one or more centers of from 10,000 to over 1,000,000 people. Despite the fact that the rural-nonfarm population differs noticeably from the urban population in some respects, the majority of the rural-nonfarm people are clearly persons with urban orientations and associations.

The data which follow are used to test the hypotheses that the rural-nonfarm population was primarily concentrated in the immediate vicinity of urban places in the two decades 1930 to 1950 and that the greater part of all rural-nonfarm growth in this period took place in such areas of urban dominance. Sociologically viewed, it is suggested, such growth is in the main an aspect of the growth and expansion of urban places. Failure to understand this can lead to serious errors in the interpretation of statistical series on urban growth.

### CENSUS AREAS

The increasing concentration of the United States population in and around the larger urban centers, especially those of metropolitan size, has already been well documented.

\* Read at the annual meeting of The Population Association of America, April, 1959.

So has the dispersal of population within metropolitan areas.<sup>1</sup> The more rapid growth of central cities in the earlier decades of this century has been replaced by more rapid rates of growth in outlying suburbs and in unincorporated territory within and adjacent to standard metropolitan areas.<sup>2</sup> A considerable amount of variation in the pattern of growth will be found among the 168 separate SMA's in the continental United States delimited by the 1950 Census, reflecting differences in their size, ecological type, and regional location. Yet the aggregate tendency toward an increasing concentration of population in such areas and toward the spread of population into formerly more rural sections of the metropolitan areas is a strong one, reflected in varying degrees by nearly all of the nation's SMA's. From 1940 to 1950 the population of these areas increased by 15.2 million persons, or 22 per cent. During this decade the population of central cities increased by 13.9 per cent, slightly less than the national rate of growth of 14.5 per cent. At the same time the outlying parts of the standard metropolitan areas showed a gain of over nine million persons, or 35.5 per cent. In contrast, nonmetropolitan areas gained only 6.1 per cent.<sup>3</sup>

In the first half of the current decade, specifically from April 1950 through April 1955, the trends of the previous decade were intensified. In this five-year period the population of the United States showed an increase of 11.8 million. All but 300,000 of this gain occurred in standard metropolitan areas. Within the SMA's the central cities grew by 3.8 per cent while the remaining territory increased by 27.8 per cent, or seven times as fast. Within the outlying territory, the urban population rose by 19.1 per cent in the five-year period while the rural population showed an increase of 46.5 per cent.<sup>4</sup>

<sup>1</sup> Conrad Taeuber and Irene Taeuber, *The Changing Population of the United States*, New York: Wiley, 1958, Chapters 5-7; Otis Dudley Duncan and Albert J. Reiss, Jr., *Social Characteristics of Urban and Rural Communities, 1950*, New York: Wiley, 1956, Chapters 11-15.

<sup>2</sup> Amos Hawley, *The Changing Shape of Metropolitan America*, Glencoe, Ill.: Free Press, 1956.

<sup>3</sup> *Census of Population: 1950*, Washington: 1953, Vol. 2, Part 1, p. 28.

<sup>4</sup> "Civilian Population of the United States, by Type of Residence: April 1955 and 1950," *Current*

These trends have led to increasing dissatisfaction with traditional rural-urban census definitions for many purposes. It is not the intention of this paper to discuss the various proposals for modifying these definitions or for substituting for what now exists some alternative such as a metropolitan-nonmetropolitan classification or one based on several size-classes of places or on occupation rather than residence. The staff of the Census Bureau has spent a great deal of time on these issues<sup>5</sup>—and clearly will continue to do so. The use of the new definition of "urban" in the 1950 Census and, associated with this, the introduction of the "urbanized area" concept and the delimitation of a sizeable number of unincorporated places constitute results of such re-examination of definitions and attempt to bring such definitions more fully into line with reality.

The definition of "rural-nonfarm" still suffers from the fact that it describes a residual category, the residential group which is left over once the urban population and the rural-farm population have been identified. As a residual category, its meaning tends to be more negative than positive. The use of the word "rural" as part of the term continues to produce some degree of semantic confusion, and although the inaccurate equation of the rural-nonfarm with the village population, which was common 15 years ago, has now been largely corrected, the basic fact that rural-nonfarm people are primarily urban people, sociologically and economically, is not widely understood.

The new definition of urban, used in the 1950 Census and to be continued in 1960,

*Population Reports*, Series P-20, No. 63 (November 2, 1955).

<sup>5</sup> See, e.g., Leon E. Truesdell, "Problems Involved in the Classification of Population as Farm and Nonfarm," *Rural Sociology*, 12 (December, 1947), pp. 419-423; Truesdell, "The Development of the Urban-Rural Classification in the United States: 1874 to 1949," *Current Population Reports*, Washington: 1949, Series P-23, No. 1; Calvin L. Beale, "Farm Population as a Useful Demographic Concept," *Agricultural Economics Research*, 9 (July, 1957), pp. 1-7; Louis J. Ducoff, "Classification of the Agricultural Population of the United States," *Journal of Farm Economics*, 37 (August, 1955), pp. 511-523; "Report of the Work Group on Census Areas and Residential Categories to the Committee on the 1960 Census of the Population Association of America," 1958 (mimeographed).

redefined as part of the urban population some 7,500,000 persons who lived in urban-fringe areas and in unincorporated places of 2,500 or more people. Most of these would have been classified as rural-nonfarm under the old definition of the 1940 Census.<sup>6</sup> This change of definition has thus increased the homogeneity of the rural-nonfarm group. Nevertheless, the situation remains far from satisfactory with the inclusion in a single residence category of persons, on the one hand, who live in villages of a variety of residential, commercial, and industrial types and, on the other, of individuals living outside urbanized and other built-up areas, ranging from remote open-country locations in Wyoming to the edges of great metropolitan complexes like Chicago, and who do not live on Census-defined farms.

#### RURAL-NONFARM CHANGES

Rural-nonfarm population totals in 1930, 1940, and 1950 were obtained from the appropriate Census volumes for each of the more than 3,000 counties in the United States with such population reported.<sup>7</sup> This was done to allow a nationwide test of the hypotheses that the rural-nonfarm population was heavily concentrated immediately around urban places in 1930 and that the degree of such concentration was heightened between 1930 and 1950, with the greater part of all rural-nonfarm increase occurring in areas of direct urban dominance.

Each county in the United States has been placed in one of nine groups, based on the size of the largest center within the county in 1940, as shown in Table 1. Group 1 is composed of the four counties in which cities

of more than a million population were located and which were not entirely urban. The remaining groups are composed of counties which contained largest centers of successively smaller sizes. Thus, counties in Group 8 had a center with at least 2,500 people but none as large as 10,000. Group 9 consists of over 1,200 counties which had no place with as many as 2,500 people in 1940 and which were almost entirely rural. In order to trace the experience of the same counties (grouped as indicated above), it was necessary to establish the groups in terms of size of largest center in some particular year. Since it was the middle year, 1940 was chosen for this purpose. The data reported for all years or decades are therefore classed according to a system related to the size of largest center in 1940.

In order to maintain comparability with the earlier periods, the rural-nonfarm population, tabulated according to the old definition, has been used in 1950 where rural-nonfarm totals differed under the old and the new definition. This allows a comparison of changes in the 1930-1940 decade with those in the following decade without the distortion which would result from a shift in the definition of the rural-nonfarm population. At the same time, this procedure results in an increase in the size of the rural-nonfarm population in 1950 in several groups over what it would be if the new definition were used.<sup>8</sup> The exact differences have not been calculated; however, the Bureau of the Census has reported that the change in definition resulted in a net shift to the urban population of five per cent of the total population of the United States. This means that some 7,500,000 fewer persons were classed as rural under the new definition than would have been so classed under the old. The majority of these were apparently rural-nonfarm persons. In most states only a few counties are affected by the change in definition, and these are largely counties which form part of a standard metropolitan area. Use of the new definition would most sharply change the rural-nonfarm figures in Groups 1 through

<sup>6</sup> Under the old urban definition some open-country residents were classed as "urban," particularly in New Hampshire, Massachusetts, and Rhode Island, in those cases where entire townships were counted as urban by special definition. This provided a slight compensation in a limited number of areas for the more general "under-definition" of the urban population. See W. R. Gordon and A. A. Asadorian, *New Americans in Rural Rhode Island*, Kingston: Rhode Island Agricultural Experiment Station, 1946, p. 5 (AES Bulletin No. 298).

<sup>7</sup> In 1940 there were 3,056 such counties. The exact number varies slightly from decade to decade because of the creation or abolition of a few counties and also because of changes in Census Bureau rulings which shift designations from urban to rural-nonfarm, or the reverse, in specific cases.

<sup>8</sup> Leon Truesdell, on the basis of his long experience in the Census Bureau, has suggested in discussion that 3,000,000 rural-farm persons were perhaps erroneously reported as rural-nonfarm in 1950.

TABLE 1. CHANGES IN THE RURAL-NONFARM POPULATION, 1930-1950, UNITED STATES SUMMARY SHEET

Group	Size of Largest Center (1940)	No. of Counties with such a Center	U. S. Rural-Nonfarm Population				
			1930	1940	1950 (old definition)	Absolute Increase 1930-1940	Absolute Increase 1940-1950
1	1,000,000 or over	4 <sup>a</sup>	382,406	565,464	1,186,299	183,058	620,835
2	500,000-999,999	7 <sup>b</sup>	572,825	661,887	955,464	89,062	293,577
3	250,000-499,999	20 <sup>c</sup>	682,347	946,889	1,608,713	264,542	659,553
4	100,000-249,999	52 <sup>d</sup>	1,703,752	2,082,902	3,642,231	379,150	1,559,329
5	50,000- 99,999	75	1,677,069	2,092,477	3,265,974	415,408	1,173,497
6	25,000- 49,999	140 <sup>e</sup>	2,282,264	2,743,339	4,194,931	461,075	1,451,592
7	10,000- 24,999	373 <sup>f</sup>	4,345,119	4,926,360	7,174,584	581,241	2,248,224
8	2,500- 9,999	1,133 <sup>g</sup>	7,035,577	7,796,001	10,492,603	760,424	2,696,602
9	Under 2,500	1,252 <sup>h</sup>	4,640,341	5,227,278	6,148,926	586,937	921,648
—	All sizes (total)	3,056 <sup>i</sup>	23,662,710	27,042,597	38,669,725	3,720,897	11,624,857

<sup>a</sup> Includes Alameda County, California (contiguous cities of San Francisco-Oakland have a combined population in this size class). Six other counties are wholly urban: San Francisco, Cal.; Bronx, Kings, New York, Queens, N. Y.; and Philadelphia, Penn.

<sup>b</sup> Includes Hennepin-Ramsey counties, Minnesota (contiguous cities of Minneapolis and St. Paul have a combined population in this size class). Suffolk County, Mass. (Boston), and the District of Columbia are wholly urban and are therefore omitted.

<sup>c</sup> Includes De Kalb County, Georgia, in which part of Atlanta City lies, and Hudson County, New Jersey, classed by the Census as all urban in 1930, as containing 2,271 RNF population in 1940 and 2,173 RNF population in 1950 (old definition). Omits Denver County, Colorado, and Orleans Parish, Louisiana, both wholly urban.

<sup>d</sup> Includes Chesterfield and Henrico Counties, Virginia, both "containing" parts of Richmond City. Omits Richmond County, New York (Staten Island), wholly urban.

<sup>e</sup> Omits Arlington County, Virginia, with 26,264 R-NF population in 1930 but classified by special Census ruling as wholly urban in 1940 and 1950.

<sup>f</sup> Omits Bristol County, Rhode Island, wholly urban in 1940 and, under the old definition, in 1950.

<sup>g</sup> Omits Nantucket County, Mass., wholly urban in 1940 and, under the old definition, in 1950.

<sup>h</sup> Includes parts of Yellowstone National Park in Montana and Wyoming not in any county.

<sup>i</sup> An additional 14 counties are entirely urban or so classified by special ruling (see previous footnotes).

4, and in all cases would lower the indicated absolute and percentage increases because of the reclassification of formerly rural-nonfarm people as urban.

As shown in Table 1, there was a total rural-nonfarm population of about 23,700,000 in 1930, the first year in which census data appeared broken down into rural-farm and rural-nonfarm by counties. An increase of three and three-fourths million persons in the decade ending in 1940 and a further increase of over three times that of the preceding ten years by 1950 resulted in a total rural-nonfarm population of nearly 39,000,000 in the latter year. In both decades the rural-nonfarm population showed a net increase in all nine county groupings and these increases were larger without exception in the decade ending in 1950 than in that ending in 1940. Absolutely, counties with one or more small urban centers (Group 8) contained the largest rural-nonfarm population of any single grouping in 1930, 1940, and

1950 and also showed the largest net gains in both decades studied. Group 9 counties, which are almost wholly rural, were second by a small margin both in absolute numbers of rural-nonfarm people in 1930 and 1940 and in absolute increase in that decade, but by 1950 had been replaced in both categories by Group 7 counties, those with at least one center between 10,000 and 24,999 population. In fact, in absolute increase between 1940 and 1950, the wholly rural counties fell behind Groups 8, 7, 4, 6, and 5 in that order.

The significance of these figures is made clearer by an examination of Table 2, in which percentage changes are shown. An inspection of the percentage increase from 1930 to 1940 indicates an irregular downward progression from counties with a city in the million class to counties which are almost wholly rural. Group 2, counties with centers between a half-million and a million, increased their rural-nonfarm population at



TABLE 2. CHANGES IN THE RURAL-NONFARM POPULATION, 1930-1950, UNITED STATES SUMMARY SHEET

U. S. Rural-Nonfarm Population								
Group	Size of Largest Center (1940)	No. of Counties with such a Center *	Percentage Increase, 1930-1940	Percentage Increase, 1940-1950	Percentage of Total RNF Increase:			
					1930-1940		1940-1950	
					Non-cumulative	Cumulative	Non-cumulative	Cumulative
1	1,000,000 or over	4 <sup>a</sup>	47.9	111.0	4.9	4.9	5.3	5.3
2	500,000-999,999	7 <sup>b</sup>	15.5	44.4	2.4	7.3	2.5	7.8
3	250,000-499,999	20 <sup>c</sup>	38.8	69.5	7.1	14.4	5.7	13.5
4	100,000-249,999	52 <sup>d</sup>	22.3	74.9	10.2	24.6	13.4	26.9
5	50,000- 99,999	75	24.8	56.1	11.2	35.8	10.1	37.0
6	25,000- 49,999	140 <sup>e</sup>	20.2	52.9	12.4	48.2	12.5	49.5
7	10,000- 24,999	373 <sup>f</sup>	13.4	45.6	15.6	63.8	19.4	68.9
8	2,500- 9,999	1,133 <sup>g</sup>	10.8	34.6	20.4	84.2	23.2	92.1
9	Under 2,500	1,252 <sup>h</sup>	12.6	17.6	15.8	100.0	7.9	100.0

\* For footnotes to this column see Table 1.

a slower rate than did the other groups down through Group 6 (centers of 25,000 to 49,999). The rural-nonfarm population in counties which were effectively entirely rural (Group 9) grew slightly more rapidly than in those with small urban places (Group 8), which are at the bottom of the list, partly perhaps because of the shifting of a considerable number of small centers out of the rural-nonfarm and into the urban category during the decade. With these exceptions, there is a clear tendency for the highest percentages of increase to occur in the county groups which contain the larger centers. The percentage increase of the four counties with million-class cities was, in fact, nearly five times as great as that of the counties with the smallest urban places.

In the following decade the percentage increase in rural-nonfarm population in the four counties with the largest centers was 110 per cent, well ahead of any other group and over six times the rate of increase in the bottom-ranking rural counties.<sup>9</sup> The next largest rate of increase, 75 per cent, occurred in Group 4, counties containing cities between 100,000 and 249,999 population. Group 2 counties again lagged behind all other groups through Group 7 in percentage

increase,<sup>10</sup> and for reasons which are not clear. With these exceptions, there is again an irregular downward progression in the percentage of increase of the rural-nonfarm population, with a steady decline from Group 4 through Group 9.

The last four columns of Table 2 show the percentage of rural-nonfarm increase for the United States as a whole which occurred in each group of counties in the two decades involved. In both decades about five per cent of the total growth occurred in only four counties, each of which was the site of a city of over 1,000,000 people. The cumulative percentages are especially useful in testing the hypotheses posited above. They show that in both decades approximately one-quarter of the rural-nonfarm increase occurred in those counties which contained a city of 100,000 or more people and which constituted only 2.6 per cent of all counties; and that between 35 and 37 per cent of such growth took place in the five per cent of all counties with a center of 50,000 or more population. Although fewer than ten per cent of the counties in the United States had centers of 25,000 or more people, these counties were responsible for almost 50 per cent of the nonfarm increase. Similarly, around two-thirds of the growth both between 1930 and 1940 and between 1940 and 1950 is found in the 22 per cent of all counties with centers of 10,000 or more population. There are about 1,800 counties in the United States

<sup>9</sup> These figures demonstrate the necessity for the change in Census definition which in 1950 brought a substantial proportion of these persons into the urban category. The cities involved are Chicago, Detroit, Los Angeles, and San Francisco-Oakland. Other cities of over 1,000,000 in 1940 are in counties which are entirely urban.

<sup>10</sup> Through Group 6 in 1930-1940.

with an urban place (Groups 1-8) and these 59 per cent of all counties accounted for 84 per cent of the rural-nonfarm increase between 1930 and 1940 and for 92 per cent between 1940 and 1950. At the same time, the percentage of increase contributed by the 1,252 rural counties, which account for about 40 per cent of all counties, declined from 16 per cent in the earlier decade to eight per cent in the latter.

#### CONCLUSIONS

The data which have been presented appear to support the hypotheses (1) that the rural-nonfarm population in the United States is primarily concentrated in the immediate vicinity of urban places, (2) that the degree to which such concentration exists is presently increasing, and (3) that the rate of rural-nonfarm growth declines with decline in the size of nearest center. The latter decline, however, is irregular.

Additional support for these hypotheses is found in data which I have published for 1930 and 1940,<sup>11</sup> and in sample tabulations which indicate that in all likelihood these trends became more pronounced by 1950. These involve a classification of all metropolitan-area counties with a breakdown into counties which contain a central city and those which are in first-tier position to central-city counties. All these groupings showed an increasing concentration of rural-nonfarm population in 1940 over 1930 and a rate of increase nearly twice that of the national average. The 265 metropolitan-area counties accounted for well over half the national increase in that decade, and the 816 first-tier counties and central-city counties combined were responsible for two-thirds of the rural-nonfarm increase. This is a clear indication of the extent to which rural-nonfarm growth was tied in with metropolitan growth between 1930 and 1940. Since then, there appears to have been no slackening of this trend nor of the increasing concentration of the rural-nonfarm population (under the old definition) in standard-metropolitan-area counties.

<sup>11</sup> Vincent Heath Whitney, "Rural-Urban People," *The American Journal of Sociology*, 54 (July, 1948), pp. 48-54. See also Whitney, "The Rural-Nonfarm Population: Patterns of Growth in a Piedmont Area," *Social Forces*: 24 (October, 1945), pp. 81-89.

Supporting evidence is also found in two studies by John Fraser Hart. In a detailed examination of the rural-nonfarm population of Georgia in 1950, Hart concludes that "at least a third of Georgia's rural nonfarm people live in villages which are presumably more urban than they are rural. The remainder . . . is largely clustered near urban centers. In short, the rural nonfarm population of Georgia is discontinuously distributed."<sup>12</sup> In a similar study of the state of Indiana, Hart found that "The village population of Indiana represents but little more than a third of the state's rural nonfarm population. [The remainder] of the rural nonfarm population is strikingly concentrated. These people may live in the open country, but they live largely in those parts of the open country which are close to our larger urban centers."<sup>13</sup>

Available data, then, all support the hypothesis that rural-nonfarm growth has been in considerable part an outgrowth of the increase and spatial expansion of the urban population. Clearly, it has been concentrated near urban places and has been particularly noticeable around metropolitan places. Furthermore, in the 20 years observed, this concentration increased and did so most rapidly near the larger cities with minor exceptions. This analysis seems to justify the action of the Census Bureau staff in redefining "urban" to include a substantial group of persons who are in every way as urban as their counterparts within incorporated centers of urban size but who, prior to the 1950 change in definition, were misrepresented as rural by inclusion in an arbitrary statistical category which confused rather than clarified their real status and associations. It can be hoped that the definitions of "rural" and "urban" will be made still more precise by future modifications required by the continuing changes in our national life and in the resulting redistribution of population.<sup>14</sup>

<sup>12</sup> John Fraser Hart, "The Distribution of the Rural Nonfarm Population in Georgia," *Bulletin of the Georgia Academy of Science*, 13 (1955), p. 123.

<sup>13</sup> John Fraser Hart, "The Rural Nonfarm Population of Indiana," *Indiana Academy of Science*, 65 (1955), p. 176.

<sup>14</sup> Leon Truesdell has suggested that by 1970 such changes may make the rural-urban classification a meaningless one, which will require replacement. (Oral comment).

## RESIDENTIAL AND OCCUPATIONAL MOBILITY IN AN AREA OF RAPID INDUSTRIALIZATION IN NORWAY \*

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*This paper, which presents a few of the results of a study of mobility in a Norwegian community, is conceived primarily as a contribution to a new method of analysis. By coding data relative to residence and occupation at regular intervals of age and using the same time span for establishment of cohorts, it is possible to separate the influence of factors related to age and time. We may thus compare persons of the same age at different points of time who are classified with respect to specific residential categories or engaged in specific occupations, thereby measuring the influence of time on mobility. By the same logic we may compare the proportion of persons of different ages at the same points of time, reflecting the influence of age.*

THE purpose of this study is to measure the volume of residential and occupational mobility, and to analyze some of the factors affecting mobility among youth in a section of North Norway. The emphasis here is more on methodological analysis than of a substantive nature.

The Rana Region selected for the study is located on the west coast of Norway about half way up the Scandinavian peninsula, 66.5 degrees north and just a few miles below the Arctic Circle. The Region surrounds the town of Mo where the Norwegian Government has recently completed the construction of a sheet steel and rolling mill. The social and economic changes which have accompanied the period of construction and industrial growth have been the subject of extensive investigation by the Institute for Sociology at the University of Oslo. The location of the Region is shown in Figure 1.

The topography of the area is one of high mountains, deep valleys cut by rushing mountain streams, and narrow fjords extending from the sea far into the interior of the country. The climate, moderated by the Gulf Stream, makes possible a small scale agriculture on the narrow strip of land along the fjord wall or beside the streams in the mountain valleys. The remainder of the country is uninhabitable. For several centuries, the population has been supported by a mixed economy of fishing and agriculture. During the past fifty years periodic attempts by British capital to exploit the low grade iron

ore of the Region has resulted in sporadic employment of a few people in mining and ore processing. Until the current period of industrial construction, more than three-fourths of the people have been employed in farming and fishing. Since 1930, however, construction of the railway, highway, the steel mill, and the facilities necessary to meet the needs of an industrial population have attracted large numbers of the local people into construction, trade, and related occupations.

The data for the study cover the period from 1930 to 1953. This period can be divided historically into three parts. From 1930 to 1939, the remnants of isolation gradually disappeared and the economic organization of the area began to change: a main highway, connecting North and South Norway, was completed, and the construction of the railroad leading south was nearly finished before World War II. From 1940 to 1945, all Norway was occupied by the German forces. Although the work on the railroad and highway was rapidly pushed during this period with prisoner-of-war labor, the socioeconomic changes associated with the earlier breakdown of isolation in the Region almost came to a standstill. The third period, 1945 to 1953, was one of rapid expansion, with the building of the mill, housing in and around the town of Mo, and other facilities to meet the needs of a growing population. The study extends only through the construction period and does not include the time during which the mill has been in operation. Actual steel production did not begin until 1954. The full impact of industrialization cannot be assessed until production has been under way for some

\* Revised version of paper read at the annual meeting of the American Sociological Association, August, 1958.

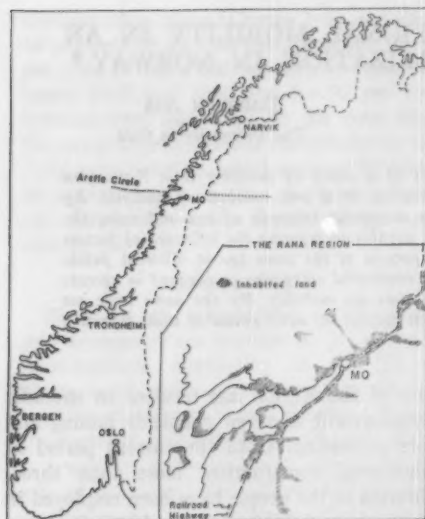


FIGURE 1. LOCATION OF MO AND THE RANA REGION IN NORWAY

time, perhaps by 1960, when the new census will make more recent data available and when the industrialization process will have proceeded much further.

#### METHOD

The sample of youth selected for the study includes 1,197 persons, 606 males and 491 females, from ten rural school districts in communities surrounding the town of Mo. The school districts were chosen to be representative of three ecological zones surrounding the town. The data were collected by school teachers in each district, who sought the information from the subjects themselves, their parents, or other close relatives. In addition to background information, a thorough residential and occupational history, from the date when the subject completed his elementary school education to the time of the report, was obtained. Only subjects completing elementary school between 1930 and 1950 were included in the sample. Since Norwegian children commonly leave the elementary school at 14, the subjects varied in age in 1953 from 17 to 37. Data on only male subjects are included in this analysis.

In order to compare the rate of migration and occupational shifts among our subjects

at different time intervals during the period 1930 to 1953, we have divided them into a series of cohorts. Each cohort includes those persons leaving the elementary school within three-year periods. The first cohort, for example, includes those graduating from 1930 to 1932 and aged 35 to 37 years in 1953; the second cohort comprises the graduates of 1933 to 1935 and aged 32 to 34; and so on. Thus the group is divided into seven cohorts. The oldest of these had 21 to 23 years to settle in residence and occupation. The youngest, graduating between 1948 and 1950, and aged 17 to 20 in 1953, were barely in the labor market and only three to five years out of school.

For each subject, residence and occupation at eight specific ages or reference points were recorded. These points are spaced at three-year intervals, starting with the first half-year out of school, 14½ years of age. In this way, we are able to show the proportion of persons in each three-year cohort classified by residential categories and by occupation at ages 14, 17, 20, and so on, up to 35. We can thus compare the residential and occupational status of persons of the same age at different points of time, or persons of different ages at the same points of time. The method of organizing the data and the residential and occupational categories are shown in Table 1.

#### THE VOLUME OF MOBILITY

*Residential.* At the time of the study (1953), only 45 per cent of the subjects resided in their home district. Twenty-two per

TABLE 1. THE ORGANIZATION AND CODING OF THE DATA \*

	1930	1931	1932	1933	1934	1935	1936	....	1951	1952	1953
Cohort I	14	15	16	17	18	19	20	-	35	36	37
	14	15	16	17	18	19	19	-	34	35	36
	(cell I)	14	15	16	17	18	19	-	33	34	35
Cohort II		14	15	16	17	18	19	-	32	33	34
		14	15	16	17	18	19	-	31	32	33
			14	15	16	17	18	-	30	31	32
				14	15	16	17	-	29	30	31

\* Each horizontal refers to the "history" of one person. Information on residence and occupation is recorded for the (circled) reference points. Persons in cell I were 14 years of age in 1931—and so on.



TABLE 2. PER CENT LIVING IN HOME DISTRICT AT GIVEN POINTS OF TIME AND AGE FOR THE SEVEN COHORTS

	AGE (reference points)								Cohort Total N = 606
	14	17	20	23	26	29	32	35	
1931 → 100	98	88	74	65	61	48	43	52	
1936 → 97	90	77	59	55	46	39		91	
1939 → 96	90	71	60	46	37			91	
1940 → 96	85	79	55	39				89	
1943 → 96	84	57	37					51	
1945 → 90	67	53						90	
1946 → 89	70							82	
1949 → 89									
1952 → 70									

cent of them lived in Mo, the steel center, while 16 per cent were scattered through the local Region, and 17 per cent had migrated beyond its limits.<sup>1</sup>

The extent of migration from the home district for each of the seven cohorts is shown in Table 2. The per cent of each cohort residing in the home district follows the tables horizontal line. Of the first cohort, for example, leaving school in the years 1930 to 1932, 100 per cent lived at home at age 14 in 1931. This proportion declines slowly to age 20, then more rapidly until by age 35 in 1952 only 43 per cent of the subjects still lived at home.

The rate of movement appears to have slowed up during the interval between ages 26 to 29, which coincide with the war period, and to have been most rapid between ages 29 and 32, coinciding with the post-war era of construction. The second cohort shows much the same pattern of movement except that the proportion of home residents was lower at each age and time period than in the oldest cohort.

The vertical columns of Table 2 show the per cent of the cohorts resident at home at corresponding ages. Although there are a few exceptions, generally the per cent of such residents is lower for each succeeding cohort at corresponding ages. This does not hold for the cohort leaving school in 1939-1941, for example, at age 20. This group included

those who graduated at the beginning of the war and whose movements appear to have been slowed up by the events associated with it. The dampening effect of the war on migration can be clearly seen if these figures are presented graphically.

One purpose of the study was to measure the attraction of the town of Mo as a magnet for migration from the Region. Therefore, we have separated the subjects residing in the town from those who have moved toward other destinations. Approximately 22 per cent of all subjects lived in Mo in 1953. The percentage was about the same for the first six cohorts, except for the youngest group where only 15 per cent had settled in the town. Since the oldest of these were under 20, the lower percentage is not surprising.

This picture of migration is quite consistent with the historical development of the town of Mo and its environment. Prior to the war it had been growing slowly as a market center for the Region. After 1945 a period of rapid construction followed, with the completion of the highway and railroad and the development of the steel complex, including housing for personnel, docks and piers, and the like. Until 1946, the proportion of persons moving toward Mo was insignificant; other destinations in the Region or elsewhere in Norway had attracted the great majority of the migrants. The attractions of the growing town after 1945 seem a little less apparent for the oldest cohort, suggesting that some of these persons were old enough to have settled in permanent residence elsewhere and less likely to terminate in Mo than the younger groups.

*Occupational.* The residential moves of these young people, for the most part, have been associated with shifts in occupation. Although a few found jobs in non-manual jobs the group as a whole was remarkably homogeneous in occupational experience. Few of the young men had the training or background which would qualify them for any work except as unskilled laborers. The principal shift, therefore, has been from agriculture and fishing toward manual labor (largely unskilled), in connection with construction and industry.<sup>2</sup>

<sup>1</sup> Residence classification utilized in this study: (1) Home district; (2) Mo (the town); (3) Elsewhere in the Region; (4) North of the Region; (5) South of the Region.

<sup>2</sup> Occupational classification utilized in this study: (1) Agriculture and fishing; (2) Manual labor—industry, construction, mining, and so on; (3) Non-

TABLE 3. PER CENT ENGAGED IN AGRICULTURE AND FISHING AT GIVEN POINTS OF TIME AND AGE FOR THE SEVEN COHORTS

	AGE (reference points)							Cohort total N = 606
	15	17	20	23	26	29	32	
1931	85	85	78	65	51	51	39	82
1936	76	73	60	42	39	32	28	91
1937	70	60	43	33	31	26		91
1940	78	65	56	38	28			89
1943	79	64	43	20				81
1946	62	43	27					90
1949	54	42						82
1952								

Some measure of the movement out of agriculture-fishing can be obtained by comparing the proportion of subjects engaged in agriculture in 1953 with the per cent of their fathers similarly occupied. While only 29 per cent of the sons were farmers, 70 per cent of the fathers were similarly employed. In Table 3 the trend in the shift from agriculture is shown in detail for each of the seven age cohorts, following the procedure depicting residential mobility in Table 2.

The steady decrease in employment in agriculture parallels the decrease in the proportions of persons resident in their home districts at successive periods of time. For each cohort there is a steady decrease with both age and time of those engaged in agriculture. There are some differences, however, in the rate at which this change takes place. The oldest group (1930-1932) started more slowly and a larger proportion at each age remained in the original occupation. The effects of the war years are again evident. Cohorts graduating between 1939 and 1944 left agriculture at a later age, (and a larger proportion of them remained in it up to the age 23) than in the case of the next older group (1936-1938). The youngest cohorts, leaving school after the war, show the steepest decline in agricultural employment: less than half of the subjects in these groups were on the farm by age 17, as compared with 85 per cent of the oldest graduates.

The differences in occupational shifting for

manual labor—retail, clerical, teaching, and so on;  
(4) Other—in school, supported, unemployed, and so on.

the various groups reflect the historical changes which have taken place in the Region over the period covered. The two oldest cohorts left agriculture at a later age than did the younger groups. Also, a larger proportion of the older age classes have remained in agriculture than of the younger cohorts. At about 25 their movement slowed up and remained stationary up to 29; these ages correspond with the beginning and the end of the war. For the graduates between 1933 and 1938 migration decreased somewhat between ages 23 and 26. The graduates of the war years (1939-1944) started more slowly but by age 23 a larger proportion had left agriculture than from the next older group. The youngest cohort has shown the most precipitous departure from agriculture.

Entrance into manual labor is the converse of leaving agriculture. The patterns for the various cohorts, and the differences between them, reflect the same influences as those discussed above with reference to Table 2. Again, the oldest group lagged behind the younger ones and their proportion in non-agricultural labor was less at each age and time period than for younger graduates. Analysis of the data shows the same slackening of rates during the war period. The tendency for mobility in the oldest group to decrease after age 30 once more suggests the influence of age in stabilizing occupational as well as residential mobility.

#### TIME AND AGE AS DETERMINANTS OF MOBILITY

We have repeatedly referred to the fact that mobility has been influenced both by the changes which have taken place in the Region over the time period covered by the study and by the aging of the subjects. By factors related to *time* (or history) we refer to influences on the rate of migration of social and economic changes in the area between 1930 and 1953. Factors related to *age* are necessarily complex, but certainly the probability of migration differs with the age of the subject—a boy at 19 is better prepared to take a job away from home than he is at 16. Stabilizing factors, such as marriage and family, commonly occurring in the late twenties in Norway, are probably obstacles to migration, while factors like increased education are likely to increase its probability.

At different ages persons also meet quite different expectations with respect to their social behavior, including the mobility expected of or permitted to them.

The method used in coding the data makes it possible to distinguish between the relative influence of these two types of factors. By keeping time and age alternately constant, and comparing the average percentage change in residence and occupation over standard time periods (six years) associated with each factor separately, we can obtain indices which measure the relative effect of each on mobility. For example, from Table 2 we can obtain nine overlapping sub-tables (3 x 3), each of which gives two indices showing the effect of age and time at specific age and time periods. The sub-table circumscribed by the years 1937 to 1943 and the ages 14 to 20, shows that the average decrease in home residence between people of the same age over this period was seven per cent. The average decrease between ages 14 and 20 was 17 per cent. Therefore, we may conclude that, in this period and over this age range, age as a factor in migration was about two and one-half times more important than the influence of change over time.

Indices obtained in this way can be used for three different comparisons: (1) The total relative importance of time and age for each of the residential and occupational categories may be compared. (2) The values and direction of the two indices may be compared for different periods of time and age for each residential and occupational category. (3) Comparisons across residential and occupational categories give some indication of the total interaction between the two factors. This paper, however, is restricted to the analysis of the influence of time and age on the residential shift away from the home district and to the town of Mo, and the occupational shift away from agriculture and fishing and into manual labor.

*Time and age as factors in residential mobility.* Table 4 shows the influence of time and age on movement away from the home district and into the town of Mo. Before comparing their influence, a note on the over-all effect of these two factors on mobility in general is in order. On the whole, the influence of age and time seem to have been about equal on residential mobility to all five

TABLE 4. INDICES REFLECTING THE INFLUENCE OF TIME AND AGE ON RESIDENTIAL MOBILITY\*

A) Change in Per cent Living in Home District

Average change over TIME		Average change over AGE	
1937-43	- 6.8	- 17.1	
	- 3.7	- 18.3	14-20
1940-46	- 6.1	- 22.8	
	- 13.5	- 21.8	
1943-49	- 12.5	- 20.4	17-23
	- 12.4	- 22.6	
	- 21.1	- 13.7	20-26
1946-52	- 21.5	- 16.1	
	- 20.9	+ 2.6	23-29

B) Change in Per cent Living in Mo

Average change over TIME		Average change over AGE	
1937-43	+ 0.7	+ 1.8	
	+ 2.4	+ 2.2	14-20
1940-46	+ 3.7	+ 3.6	
	+ 10.7	+ 7.0	
1943-49	+ 11.7	+ 8.0	17-23
	+ 13.4	+ 7.7	
	+ 12.6	+ 5.7	
1946-52	+ 13.9	+ 5.4	20-26
	+ 13.2	+ 0.4	23-29

\* A negative index indicates a decrease of the percentage over the years (time or age). The broken lines connect the indices based on the same sub-tables.

destinations (see footnote 1). Of the total of 45 possible comparisons (nine comparisons for each of five destinations), the age index is the larger in 24 instances, the time index in 20, and in one case their influence is identical.

Migration to other points in the Region and to South Norway is clearly dominated by the age factor. This is what one would expect. Migration within the Region (except toward Mo) has been largely a farm-to-farm movement or a shift from agriculture to mining or unskilled work on roads or railroad. These short jumps are largely influenced by age. Migration to South Norway seems to have occurred at about a regular rate as individuals become old enough to leave home.

Comparison of the migration from the home district (Table 4A) with movement toward the town (Table 4B) shows striking differences. The former indicates the greatest change along the *age dimension*: seven out of the nine comparisons reveal age to be exerting a stronger influence than time. Movement toward Mo, however, is clearly dominated by changes along the time dimension.

Changes in the influence of the two factors follow much the same pattern for all residential categories. Along the age dimension the greatest changes have taken place since the war, although its influence increases steadily with the lapse of time. It would appear then that the changes in mobility before and during the war were most closely related to age while those occurring since the war have been related to the great changes in the area which have taken place since its termination, changes which have been associated with the establishment of industry in Mo.<sup>3</sup>

While space limitations preclude presentation of all the data, it is clear that the destinations of migrants from this Region have changed over the years between 1930 and 1953. Long distance migration—this is, movement toward the south and abroad—which was dominant before the war, has decreased while northward movement has tended to increase. During the post-war period, with the construction of the steel mill and the consequent demand for labor, other destinations have held less attraction.

*Time and age as factors in occupational mobility.* In the Rana Region two broad occupational categories have been predominant, agriculture and manual (industrial and construction) labor. Throughout the period covered by the study the major shift has been between these two types of occupation. The influence of *time* and *age* on this shift is shown in Table 5.

<sup>3</sup> The method of analyzing migration used in this study appears to have some bearing on "the problem of generations" as developed by Heberle. It is not only the stage in the life cycle, but also the differential experience of different generations, especially in periods of rapid social change, which is significant in determining role. Rudolph Heberle, *Social Movements*, New York: Appleton-Century-Crofts, 1951, Chapter 6, esp. pp. 124-217. See also Karl Mannheim, "Das Problem der Generationen," *Kölner Vierteljahrs Hefte für Soziologie*, 7 (1928), p. 311.

TABLE 5. INDICES REFLECTING THE INFLUENCE OF TIME AND AGE ON OCCUPATIONAL MOBILITY \*

A) Change in Per cent Engaged in Agriculture and Fishing

	Average change over TIME	Average change over AGE	
1937-43	- 11.2	- 15.2	
	- 5.2	- 19.7	14-20
1940-46	- 10.7	- 17.5	
	- 15.6	- 16.8	
1943-49	- 8.4	- 19.3	17-23
	- 7.9	- 19.3	
	- 21.8	+ 7.3	20-26
1946-52	- 18.5	- 10.3	
	- 16.9	+ 6.2	23-29

B) Change in Per cent Engaged in Manual Labor

	Average change over TIME	Average change over AGE	
1937-43	+ 11.5	+ 14.2	
	+ 2.2	+ 15.8	14-20
1940-46	+ 6.5	+ 19.6	
	+ 11.8	+ 15.2	
1943-49	+ 12.3	+ 18.2	17-23
	+ 15.8	+ 21.9	
	+ 24.4	+ 6.7	20-26
1946-52	+ 22.9	+ 7.4	
	+ 19.8	- 3.7	

\* A negative index indicates a decrease of the percentage over the years (time or age). The broken lines connect the indices based on the same sub-tables.

The decline in the proportion of subjects in agriculture (and the increase in manual labor) is shown above in Table 3 for each of the seven cohorts. The influence of age on this process seems to have been highest among the "very young" (14 to 20) and the "young" (17 to 23). If at the age of 25 or older a person has not left agriculture, the probability of such a move is relatively low. In fact,



as shown in table 5A, there seems to have been some shift back toward agriculture in the age group 23 to 29. Again, age-related factors appear to have been more important during the pre-war and war periods, while those related to time and historical change became most significant during the war period and the rise of industry.

#### CONCLUSION

In conclusion we should like to refer to a point recently made by Thomas.<sup>4</sup> She emphasizes that the one generalization which can be established about the selectivity of migration concerns the relationship between age and migration and the preponderance of youth among migrants. She further points out that temporal factors are often important in determining the nature and strength of age selection. An important limitation of most studies of the selectivity of migration is that

<sup>4</sup> Dorothy Swaine Thomas, "Age and Economic Differentials in Interstate Migration," *Population Index*, 24 (October, 1958), pp. 313-324.

they are limited to comparisons of migrants with non-migrants, or with the general population, at points of origin or destination and at given points of time. Thus they throw little light on changes in the characteristics of migrants over periods of time.<sup>5</sup> This is the problem which we have attempted to attack in this study. Even though limited by its small scale, we have suggested a method whereby changes in the volume of migration, age at time of migration, origin and destination, and occupations of migrants may be related to socio-economic changes taking place in an area undergoing rapid transformation. It is hoped that the method will stimulate further studies on a larger scale by students who are interested in this problem.

<sup>5</sup> See C. T. Pihlblad and C. L. Gregory, "Selective Aspects of Migration Among Missouri High School Graduates," *American Sociological Review*, 19 (June, 1954), pp. 314-324; and Pihlblad and Gregory, "Occupation and Patterns of Migration," *Social Forces*, 36 (October, 1957), pp. 56-64. See also Maurice Benewitz, "Migrant and Non-migrant Occupational Patterns," *Industrial and Labor Relations Review*, 9 (January, 1956), pp. 235-240.

### THE CONCEPT OF SOCIAL MOBILITY: AN EMPIRICAL INQUIRY

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*A common use of the term "social mobility" implies that there exist high intercorrelations among the several dimensions presumed to comprise the concept. A comprehensive statistical analysis of 22 variables presumably measuring various aspects of social movement shows that even when the focus of observation is restricted to the married pair at least eight, and possibly nine, orthogonal dimensions can be isolated. Among other things, the data show that it is erroneous to posit interchangeabilities among or between objective and subjective dimensions of mobility; of husband and wife variables; and of intergenerational and intragenerational mobility. Thus the most general implication of this paper is that "social mobility" is a complex multidimensional concept consisting of an indeterminate but considerable number of components.*

STUDENTS of social mobility, like those concerned with other social phenomena, seek to establish dependable empirical generalizations and systematic theoretical formulations. Foremost in the procedure, arising logically and sequentially prior to

hypothesis-testing and theory-construction, is the formation of adequate classificatory concepts. In this paper, we briefly survey current attempts to deal with the concept of social mobility and offer an alternative approach in the form of a comprehensive

statistical analysis of 22 variables presumably measuring different aspects of social movement.<sup>1</sup>

In its most general form our inquiry may be defined as follows: If we take the married couple as the basic unit of analysis, what are the *minimum* number of factors which must be included in a classificatory concept capable of accounting for mobility behavior? Implicit in this formulation is the critical methodological query concerning the extent to which a large number of potential indicators of social mobility may be safely treated as interchangeable.

#### THE CURRENT STATUS OF THE CONCEPT OF SOCIAL MOBILITY

Sociologists exhibit a gratifyingly high degree of consensus in their conception of the most general meaning of the term social mobility. In his recent text, Barber offers a definition which in substance is similar to formulations appearing in nearly all treatises devoted to this topic: "We have been using the term social mobility to mean movement, either upward or downward, between higher and lower social classes; or more precisely, movement between one relatively full-time, functionally significant social role and another that is evaluated as either higher or lower."<sup>2</sup>

It is evident that the term social mobility, defined in this fashion, performs the useful function of a "sensitizing concept," unifying research and directing attention to a broad field of inquiry. But pending the further elaboration of "movement" and "social

class," two of the ambiguous terms in the preceding definition, the concept of social mobility so framed cannot serve as an analytical tool.

#### THE CONCEPTUALIZATION OF MOVEMENT

The research on mobility performed during the past three decades has been highly diverse both in approach and quality, but it is possible to discern a developing pattern of alternative solutions to the following five fundamental problems in the conceptualization of "movement":

1. *The Unit of Analysis.* Emphasis in American mobility research has been alternately directed to the individual, the family, the entire society, or all three as the basic unit of analysis.<sup>3</sup> By far the greatest number of studies have been undertaken from a societal perspective and have proceeded from a value premise favoring an open-class system and a high degree of permeability in the barriers separating socio-economic strata. As a result, the bulk of empirical investigations has been concerned primarily with ascertaining whether vertical mobility is increasing or decreasing within the total society, within some specific community, or within some designated occupational category or social stratum.<sup>4</sup> An increasing number of

<sup>3</sup> For illustrations of all three approaches see Lipset and Bendix, *op. cit.*

<sup>4</sup> Illustrations of this type of emphasis may be found in Gideon Sjoberg, "Are Social Classes in America Becoming More Rigid?" *American Sociological Review*, 16 (December, 1951), pp. 775-783; William Petersen, "Is America Still the Land of Opportunity?" *Commentary*, 16 (November, 1953), pp. 477-486; J. O. Hertzler, "Some Tendencies Toward a Closed Class System in the United States," *Social Forces*, 30 (March, 1952), pp. 313-323; Natalie Rogoff, *Recent Trends in Occupational Mobility*, Glencoe, Ill.: Free Press, 1953; S. M. Lipset and Natalie Rogoff, "Class Opportunity in Europe and the U. S.: Some Myths and What the Statistics Show," *Commentary*, 17 (December, 1954), pp. 562-568; Ely Chinoy, "Social Mobility Trends in the United States," *American Sociological Review*, 20 (April, 1955), pp. 180-186; D. V. Glass, editor, *Social Mobility in Britain*, London: Routledge and Kegan Paul, 1954; Percy E. Davidson and H. Dewey Anderson, *Occupational Mobility in an American Community*, Stanford: Stanford University Press, 1937; Mabel Newcomer, "The Chief Executives of Large Business Corporations," *Explorations in Entrepreneurial History*, V, Cambridge: Harvard University Press, 1952-1953, pp.

<sup>1</sup> For bibliographies in the field of social mobility, see: Pitirim A. Sorokin, *Social Mobility*, New York: Harper, 1927; H. D. Laswell, D. Lerner, and C. E. Rathwell, *The Comparative Study of Elites: An Introduction and Bibliography*, Stanford: Stanford University Press, 1952; Harold W. Pfautz, "The Current Literature on Social Stratification," *American Journal of Sociology*, 58 (January, 1953), pp. 391-418; *Current Sociology*, "Social Stratification and Social Mobility," II, 1 and 4, 1953-1954; Bernard Barber, *Social Stratification*, New York: Harcourt Brace, 1957; R. W. Mack, L. Freeman, and S. Yellin, *Social Mobility: Thirty Years of Research and Theory*, Syracuse: Syracuse University Press, 1957; S. M. Lipset and R. Bendix, *Social Mobility in Industrial Society*, Berkeley and Los Angeles: University of California Press, 1959.

<sup>2</sup> Barber, *op. cit.*, p. 356.

empirical and theoretical inquiries have attempted to link social mobility with differential culture patterns, child-rearing practices, value commitments, personality syndromes, general attitude clusters, and the like, in order to delineate the consequences of these presumed correlates of social mobility for the *individual and family life*.<sup>5</sup>

It is obvious, of course, that in any concrete situation individual social mobility occurs within a specific context of societal

change, including shifting socio-economic structures, so that ultimately an adequate theory of social mobility will be required to articulate the interpenetration of all three levels of analysis—the individual, the familial, and the societal.

2. *The Direction of Movement.* The definition of direction (as opposed to its measurement) in social mobility is one of the most stable and straightforward notions in sociology. The use of the familiar terms *vertical* and *horizontal* mobility have undergone little modification since Sorokin's description in his pioneer *Social Mobility* some thirty years ago:

By horizontal social mobility . . . is meant the transition of an individual . . . from one social group to another situated on the same level.

By vertical social mobility is meant the relations involved in a transition of an individual . . . from one social stratum to another. According to the direction of the transition there are two types of vertical mobility: ascending and descending.<sup>6</sup>

### 3. *The Reference Points of Movement.*

Students of social mobility have been obliged to identify appropriate points of arrival and departure in charting the movement of individuals over time. The idea of *intergenerational* mobility involves a *person to person* comparison of the social stratum achieved by sons, fathers, and even grandfathers at comparable periods in their lives. *Intragenerational*, or career mobility, is a concept which usually has been restricted to occupational changes, and refers to the mobility of the *same individual* from the time of his first full-time job through his working lifetime.

### 4. *The Unit of Measurement in Movement.*

The distinction between the amount and distance of mobility is often wholly ignored in mobility studies. *Amount* involves the proportion of individuals who are upwardly or downwardly mobile within some stratifi-

1-33; Mabel Newcomer, *The Big Business Executive, The Factors that Made Him, 1900-1950*, New York: Columbia University Press, 1955; Suzanne I. Keller, *The Social Origins and Career Lives of Three Generations of American Business Leaders*, New York: Columbia University, unpublished Ph.D. thesis, 1953; S. M. Lipset and R. Bendix, "Social Mobility and Occupational Career Patterns," *American Journal of Sociology*, 62 (January and March, 1952), pp. 366-374, 494-504; Donald R. Matthews, "United States Senators and the Class Structure," *Public Opinion Quarterly*, 18 (Spring, 1954), pp. 5-22; William Miller, "The Recruitment of the American Business Elite," *Quarterly Journal of Economics*, 64 (May, 1950), pp. 242-253; Albert J. Reiss, Jr., "Occupational Mobility of Professional Workers," *American Sociological Review*, 20 (December, 1955), pp. 693-700; Reinhard Bendix, Seymour M. Lipset and F. Theodore Malm, "Social Origins and Occupational Career Patterns," *Industrial and Labor Relations Review*, 7 (January, 1954), pp. 246-261; Beverly Davis, "Eminence and Level of Social Origins," *American Journal of Sociology*, 59 (July, 1953), pp. 11-18; W. Lloyd Warner and James C. Abegglen, *Big Business Leaders in America*, New York: Harper, 1955; Warner and Abegglen, *Occupational Mobility in American Business and Industry, 1928-1952*, Minneapolis: University of Minnesota Press, 1955; Stuart Adams, "Origins of American Occupational Elites, 1900-1955," *American Journal of Sociology*, 62 (January, 1957), pp. 360-368.

<sup>5</sup> See, e.g., Melvin M. Tumin, "Some Unappreciated Consequences of Social Mobility in a Mass Society," *Social Forces*, 36 (October, 1957), pp. 21-37; Evelyn Ellis, "Social Psychological Correlates of Upward Social Mobility Among Unmarried Career Women," *American Sociological Review*, 17 (October, 1952), pp. 558-563; A. B. Hollingshead, R. Ellis, and E. Kirby, "Social Mobility and Mental Illness," *American Sociological Review*, 19 (October, 1954), pp. 577-584; Fred L. Strodtbeck, "Family Interaction, Values, and Achievement," in D. C. McClelland, A. L. Baldwin, U. Bronfenbrenner, and F. L. Strodtbeck, *Talent and Society*, Princeton, N. J.: D. Van Nostrand, 1958, pp. 135-194; Ely Chinoy, *Automobile Workers and the American Dream*, New York: Random House, 1955; E. E. Lemasters, "Social Class Mobility and Family Integration," *Marriage and Family Living*, 16 (August, 1954), pp. 226-232.

<sup>6</sup> Sorokin, *op. cit.*, p. 133. In this paper we follow current practice by referring to "social mobility" as if it were synonymous with the more accurate term "vertical social mobility." Considerations of stylistic convenience and standardization of terminology seem to warrant the loss of terminological precision. For a recent discussion of horizontal social mobility see Richard T. Morris and Raymond J. Murphy, "The Situs Dimension in Occupational Structure," *American Sociological Review*, 24 (April, 1959), pp. 231-239.

cation system. The *distance* of mobility, on the other hand, is a measure of the number of "steps" of upward or downward movement traversed by an individual or a group. This distinction is an important one because conclusions derived from the amount of mobility will ordinarily yield a greater impression of fluidity than results obtained by utilizing distance as the appropriate unit of measurement.

5. *The Visibility of Movement.* The solution to the four preceding problems in the conceptualization of movement may ordinarily be disposed of by the availability of appropriate data or the predilections of the investigator. This is not so with respect to the admissibility of using so-called *subjective* measures of movement (dispositions, attitudes, values) as opposed to *objective* measures (external, visible evidence of change) as mobility indicators. This is a matter involving considerable theoretical controversy and one that has been the subject of a number of recent essays and studies. The issue hinges on the extent to which hypothesized subjective dispositions favoring vertical mobility, such as those implied in the Protestant Ethic, dependably predict mobility achievement; and conversely, the extent to which a change in life chances, such as increase in income, will produce appropriate changes in attitudes and values. Although the evidence is sparse on these points most pertinent research asserts that there is a positive relationship between subjective and objective mobility.<sup>7</sup>

<sup>7</sup> The most extensive research on mobility and motivation has pointed to the differential appearance of the "achievement motive" in specific social classes, ethnic groups, and so on, without actually demonstrating the relationship of this syndrome to subsequent objective achievement. See David C. McClelland *et al.*, *The Achievement Motive*, New York: Appleton-Century-Crofts, 1953; and McClelland *et al.*, *Talent and Society*, *op. cit.* However, a major interpretative concept, anticipatory socialization, was introduced by Merton to explain the findings in *The American Soldier* that those "privates who accepted [in advance] the official values of the Army hierarchy were more likely than others to be promoted." Robert K. Merton and Paul F. Lazarsfeld, editors, *Studies in the Scope and Method of the American Soldier*, Glencoe, Ill.: Free Press, 1950. Hyman has presented convincing evidence that "reduced striving for success among the lower classes, an awareness of lack of opportunity, and a lack of

On the other hand, Reissman's findings on the relationship between aspiration level and achievement are ambiguous, and More found that aspiration for higher income is inversely related to upward mobility.<sup>8</sup> In summary, then, any proposition that objective and subjective indicators of movement are at all interchangeable must still be regarded as hypothetical.

#### THE CONCEPTUALIZATION OF SOCIAL CLASS IN MOBILITY STUDIES

There is a notable tendency in studies of social mobility to treat occupation as an adequate single index of social class and to employ the terms social mobility and occupational mobility interchangeably. This practice has arisen partly as a concession to methodological difficulties and partly from theoretical considerations supported by empirical evidence. On the purely practical level the use of occupation as an index offers a number of important advantages. As Kahl has pointed out:

The most practical procedure is to use a single measurement (rather than a complex index), and one that is simple and can be supplied by the son concerning both himself and his father. Furthermore, it should have relatively stable meaning from one generation to the next (and preferably, one country to another). Almost all researchers have used occupation, and they have grouped occupations into broad categories such as those of the

valuation of education, normally the major avenue to achievement of high status" hampers such persons in advancing in the American class structure. Herbert H. Hyman, "The Values Systems of Different Classes: A Social Psychological Contribution to the Analysis of Stratification" in R. Bendix and S. M. Lipset, editors, *Class, Status and Power*, Glencoe, Ill.: Free Press, 1953, pp. 426-442. See also Strodtbeck's recent contention that people who believe the world is amenable to rational mastery and who prefer individual rather than collective credit for the work they do, hold values important for achievement in the United States. F. L. Strodtbeck in McClelland *et al.*, *Talent and Society*, *op. cit.*, pp. 186-187.

<sup>8</sup> Leonard Reissman, "Level of Aspiration and Social Class," *American Sociological Review*, 18 (June, 1953), pp. 223-242. According to Reissman's data, the relationship between aspiration level and achievement was positive within the older age groups while the reverse held within the younger age groups. See D. M. More, "Social Origins and Occupational Adjustment," *Social Forces*, 35 (October, 1956), pp. 16-19.



social-economic levels of the U. S. Bureau of the Census. . . .<sup>9</sup>

The rationale for capitalizing on the convenience implicit in the use of occupation as a single index resides in two conventional propositions frequently advanced by social class theorists: (1) the assumption in *static* analysis that there exist high intercorrelations among the several indices traditionally assumed to represent social class,<sup>10</sup> and (2) the assumption in *dynamic* analysis that changes in any of the class indices will be accompanied by changes of comparable magnitude in all of the others. It is difficult to quarrel with the first of these postulates; indeed its validity is a necessary precondition for speaking meaningfully of social class. It is by no means clear, however, that the assumption of the intercorrelation of indices underlying the static analysis of stratification systems is directly exportable to the study of dynamic processes of social mobility. Critics have asserted that occupational mobility involves only one area of potential change, movement in the prestige hierarchy, and that a substantial number of additional dimensions must be examined before it becomes safe to regard social mobility and occupational mobility as even approximately synonymous.<sup>11</sup>

<sup>9</sup> Joseph A. Kahl, *The American Class Structure*, New York: Rinehart, 1957, pp. 252-254.

<sup>10</sup> For impressive empirical confirmation of this assumption, see J. A. Kahl and J. A. Davis, "A Comparison of Indexes of Socio-Economic Status," *American Sociological Review*, 20 (June, 1955), pp. 317-325. The authors show in their factor analysis of nineteen measures of socio-economic status that the Warner Occupational Scale has a loading of .88 on a general factor of socio-economic status and that the much maligned Edwards Scale is scarcely less efficient.

<sup>11</sup> Such dimensions as power and decision-making opportunities, social status, reference-group behavior, possibilities for use of skills and creative expression, self-imagery, income, and others have been variously mentioned by, among others, Lipset and Bendix, *Social Mobility in Industrial Society*, *op. cit.*, Introduction and Chapters 8, 9, and 10; S. M. Miller, "The Concept of Mobility," *Social Problems*, (October, 1955), pp. 65-72; Peter M. Blau, "Social Mobility and Interpersonal Relations," *American Sociological Review*, 21 (June, 1956), pp. 290-295; M. M. Tumin and A. S. Feldman, "Theory and Measurement of Occupational Mobility," *American Sociological Review*, 22 (June, 1957), pp. 281-288.

An illustration of this point of view may be found in the caution that changes in occupational status may not be accompanied by a comparable movement in reference group identification. Thus Blau notes that "persons upwardly mobile in the occupational hierarchy who continue to associate largely with working class people, and downwardly mobile persons who continue to associate mostly with middle class people, have changed their economic position but not their social affiliation."<sup>12</sup> In like manner, S. M. Miller poses the question "Are we automatically including changes in income or in skill level when we measure changes in occupational prestige level?" In reply, he considers eight possible combinations of potential simultaneity of movement and speculates that in "only two of the eight conditions do all three indicators move simultaneously in the same direction."<sup>13</sup> The logical status of Blau's and Miller's reservations are identical: both cast doubt on the desirability of treating social mobility as a unidimensional concept.<sup>14</sup>

Clearly the propriety of using occupational mobility as a single index of social mobility depends on a variety of unverified assumptions about the existence and implications of the intercorrelations of occupational and other indices of movement. It appears equally evident that the solution to these problems awaits the results of direct empirical inquiry.

In the light of the foregoing considerations, it is evident that numerous critical issues are still unsettled. Hence, sociologists who in the present state of knowledge aspire to develop

<sup>12</sup> Blau, *op. cit.*, p. 293.

<sup>13</sup> S. M. Miller, *op. cit.*, p. 67.

<sup>14</sup> A case can be made, too, for considering other dimensions in occupational movement even if we make the counter-assumption that social mobility is a unitary concept. For instance, in a forthcoming monograph on factory workers in India, Richard Lambert finds a high positive relationship between ascending occupational mobility and favorable attitudes towards the company. But as Lambert recognizes, since the occupational strata were formally specified according to broad skills carrying differential prestige and monetary rewards, any one of these three highly intercorrelated variables might contribute to the favorable response to the management. The caveat here is that if the monetary side of mobility, for example, is neglected the very structure of the classificatory concept will distort the interpretation of the phenomenon being observed—this despite the fact that the unitary character of the classificatory concept is not under dispute.

a theory of familial social mobility would be obliged to undertake the formidable task of completing all the cells resulting from the combinations of the many dimensions which have been mentioned. Now, it would be an undeniable advantage from the standpoint of scientific elegance and economy if the number of dimensions could be sharply reduced. Such reduction could be justified only under conditions in which the several indicators of social mobility were interchangeable, that is, if their intercorrelations were sufficiently high to permit the substitution of one for the other. There is *prima facie* indication that several sets of dimensions are probably logically irreducible: the distinctions between vertical and horizontal mobility, upward and downward movement, the amount and distance of mobility, and individual, familial, and societal mobility. Accordingly, the prospect of achieving a parsimonious reduction of dimensions rests largely on the possibility of demonstrating interchangeabilities: (1) among or between objective and subjective dimensions of movement; (2) husband and wife variables; and (3) intergenerational and intragenerational mobility.

#### THE DATA

Although the materials for this analysis were collected for a study of fertility, social mobility was the object of special attention.<sup>15</sup> An attempt was made to collect data relevant to a wide variety of aspects of mobility, which amounted finally to no less than 22 measures.

The sample interviewed is especially suitable for an analysis of mobility. Native-white, married couples totalling 1,165, were selected at random from listings of birth records throughout the largest standard metropolitan areas of the country.<sup>16</sup> Since the fertility study concentrated on the interval between the second and third child (a longitudinal design features a complete first

reinterview in early 1960), all couples sampled had had their second child approximately six months before the first interview. This is an important consideration for any analysis of mobility: with stage of family growth or life cycle held constant, a major source of extraneous considerations is eliminated.

The sample consists, then, of couples living in the largest urban and suburban areas of the nation who were in their middle and upper twenties at the time they were interviewed (1957), had been married for an average of between five and six years, and who all recently had had their second child.

#### THE MEASURES OF MOBILITY

1. *Occupational Mobility.* Of the 22 measures of mobility, a total of six relate to change in occupational status or job. The simplest of these is number of job changes since marriage (No. 6). More explicitly embodying an underlying status dimension are the five measures of differences in the prestige rating (based on the familiar North-Hatt scale) of occupation at three points in time: the job held by the husband currently, his occupation when first married, and the longest occupational status held both by his and his wife's father (Nos. 1-5). Collectively, these measures are an index of intra- and inter-generational occupational mobility, with the latter delineated in terms of different reference points.

2. *Financial Mobility.* Three variables relate directly to income change. The trend of the family's income during the past year (1956) is measured by a simple qualitative response (No. 7). Change in the husband's earnings is the absolute difference in his earnings during his first year of marriage and his current earnings (No. 8). Change in family income is measured in the same way but includes all other sources of income—income from rents, insurance, return on investments, and income the wife may have earned—as well as the husband's earnings (No. 9).<sup>17</sup>

3. *Residential Mobility.* This dimension is indexed by two variables—number of moves since marriage (No. 10) and length of time the couple has lived in their current

<sup>15</sup> The fertility study is being conducted by the Office of Population Research, Princeton University, under the administrative direction of the Milbank Memorial Fund. It is supported by grants from the Carnegie Corporation and the Population Council, Inc.

<sup>16</sup> Chicago, Detroit, Los Angeles, New York, Philadelphia, Pittsburgh, and San Francisco.

<sup>17</sup> Fewer than seven per cent of the wives interviewed were working currently.

residence (No. 11). The emphasis here is on changes in housing whether or not they are accompanied by changes in community of residence.

4. *Mobility Perceptions and Aspirations.* Exactly one-half of the variables relate to perceptions, statements of values, and aspirations.<sup>18</sup> Five purport to measure "drive to get ahead" in the context of sacrifices the respondent feels he or she might make in order to get ahead in work or in life generally. For example, the wife's score on the "drive to get ahead" index (No. 12) measures her willingness to sacrifice the following values in order "to get ahead in life": leave friends, to become more active in community organizations and clubs not of one's own choice, to postpone having another child, to "keep quiet" about religious views, to have her husband take a job with less security but more opportunity, to move to a less pleasant neighborhood temporarily, to see husband less because he would be working more, and to send children to a school of lesser quality. Her and her husband's willingness to sacrifice ideological convictions (Nos. 13 and 19) combines items on willingness to keep quiet about political and religious views in order to get ahead. The wife's willingness to sacrifice "social interests" to get ahead (No. 14) combines an item on "entertaining people only because they were connected with her husband's work" with an item on becoming more active in incompatible organizations and clubs.<sup>19</sup>

Another approach is represented in the index labelled "social status aspirations" (No. 15). The five items in this index aim specifically at measuring the wife's ambition for social prominence. Whether or not she would be pleased by having her name appear in the society column of a newspaper is a typical item in this series.

On the assumption that many parents conceive of mobility in terms of their children's future (irrespective of frustration-displace-

ment-projection theory), a scale of the mother's aspiration for sending her children to college was developed (No. 16). Four questions relating to her expectations, plans for financing a college education, and intensity of the ambition to send a son or daughter under serious financial hardship<sup>20</sup> were included.

Perception of opportunity might reflect a certain sensitivity to mobility values. This assumption was translated into a two-question index (No. 17) in which the wife was asked to evaluate her husband's chances for getting ahead in his work and the extent to which he is finding it possible to improve his chances.

The husband's "drive to get ahead" index (No. 18) is also composed of eight items. In the questions asked of the husband, "getting ahead" was defined in terms of his work as well as life generally. Most of the items refer to the same values as for the wife, such as leaving friends, living in an undesirable neighborhood, postponing another child, not seeing spouse as much as he would like, sending children to an inferior school, and keeping quiet about religious views. The remaining two items refer to going without any vacation and risking health.<sup>21</sup>

Another approach to the measurement of the husband's aspiration level is reflected in a set of items which try to assess his perception of the importance of getting ahead (No. 20). The eight questions in the index are very direct, for example: "It is important to me to own material things, such as a home, car or clothing which are at least as good as those of my neighbors and friends."

Still another approach attempts to probe the husband's level of satisfaction with his present social status by asking whether or not he would be satisfied if his children enjoyed a similar status by the time they reached his age. Four questions relating to occupation, education, opportunity, and income and consumption are included (No. 21).

<sup>20</sup> Only two of the 1,165 mothers interviewed would send a daughter, but not a son, to college if it meant serious financial hardship.

<sup>21</sup> This approach to the study of mobility aspirations was first suggested by Reissman, *op. cit.* Although it cannot be conclusively demonstrated, there is reason to believe that these questions may be tapping a feeling of frustration and deprivation rather than ambition.

<sup>18</sup> All summary indexes underwent thorough internal item analyses prior to construction, including either factor analysis or Guttman scalogram analysis.

<sup>19</sup> These three sub-sets of items were scored separately from the parent "drive to get ahead" indices because of their extremely high homogeneities. There is a one-eighth overlap of each of these sub-scales in the parent sets.

On the assumption that the husband's perception of mobility is largely determined by his occupation and job, a series of questions was developed to tap an area termed his "commitment to work values" (No. 22). Eight questions comprise this index which ask if he sometimes regrets going into his kind of work, how much interest he has in work compared with leisure time activities, and similar items probing involvement in the work life.

#### IDENTIFICATION OF FACTORS

The statistical problem, corresponding to the preceding substantive discussion, is the reduction of many measures of mobility to as few orthogonal dimensions as possible. The assumption that mobility is not a unitary concept implies the existence of at least two uncorrelated dimensions. The questioning of the interchangeability of measures of mobility further implies that no single underlying dimension is strongly represented in each and every measure. In the language of factor analysis, there is more than one common factor, and there is no pronounced general factor among the various measures of mobility.

Apropos of this formulation of the problem, factor analysis (the centroid solution) with rotation to simple structure (quartimax criterion) was employed.<sup>22</sup> The resulting factor structure (Table 2) consists of nine uncorrelated common factors.<sup>23</sup> The separation of measures by factors is unusually well defined—distinguishing, in most instances, among nominally distinct measures of mobility. This latter feature of the factor structure encourages the supposition that some of the factors can be correctly identified from these measures alone.

The contents of the first three factors are manifestly clear. *Factor 1* is occupational mobility relative to the prestige of the husband's father's occupation. *Factor 2* is also intergenerational occupational mobility, but

from the reference point of the wife's father's occupation. *Factor 3* is intragenerational mobility; it is relative to the husband's occupation at marriage. The uncommonly high factor loadings (for example, .96 and .80 in *Factor 1*) in these first three factors border on artifact of measurement,<sup>24</sup> although they genuinely indicate that mobility measured from point X is not interchangeable with mobility measured from point Y.<sup>25</sup>

*Factor 4* displays its largest loading among the three measures of income change and clearly represents another intragenerational mobility factor—economic mobility.

*Factor 5* reveals highest loadings on variables 6, 10, and 11—number of job changes, number of moves since marriage, and length of residence in current place. Variables 10 and 11 form another pairing of the type discussed above. The most appropriate label for this factor appears to be spatial mobility.

These first five factors account for the "objective" or visible dimensions of mobility. The remaining four factors are concerned with the "subjective" dimensions of mobility (feeling states, attitudes, or values), assumed to be the more subtle analogues of visible mobility. *Factor 6* derives its name from the variable having its highest loading, Husband's Drive to Get Ahead. It contains an element of social opportunism expressed by the willingness on the part of the husband to sacrifice ideological conviction for the sake of mobility and refers also to the importance he attaches to getting ahead. *Factor 7* is the

<sup>24</sup> For example, if there were no intragenerational occupational mobility the loadings would be even greater, perhaps unities, since the two measures of mobility, say, from the husband's father's occupation, would have to correlate perfectly if measurements were reliable. Also, even with no intragenerational mobility, orthogonal factors would be isolated simply because the wife's father's and the husband's father's occupational prestige ratings do not correlate too highly. Thus the high loadings and the separation of factors are in part due to pairings of measurements by their common element (the parental occupation) and the lack of high correlation between the two parental occupations. This near artifact does not, however, detract in the slightest from the substantive reasons prompting the inclusion of the measures in the analysis.

<sup>25</sup> The intergenerational mobility of a couple may be upward or downward, depending upon the husband's or wife's father's occupation as the referent. Studies ordinarily use male lineage to measure mobility.

<sup>22</sup> Communalities were estimated by successive iterations until stability was achieved.

<sup>23</sup> The process of extracting factors ended when the last factor extracted contained no pair of elements whose product exceeded  $\frac{2}{\sqrt{N}}$  (Humphrey's rule), or approximately twice the standard error of the correlation coefficients.



TABLE 1. MATRIX OF PEARSONIAN COEFFICIENTS OF CORRELATION AMONG TWENTY-TWO MEASURES OF MORTALITY

[illegible]

TABLE 2. ROTATED \* FACTOR STRUCTURE OF TWENTY-TWO MEASURES OF MOBILITY<sup>28</sup>

No.	Measure of Mobility	1	2	3	4	5	6	7	8	9	h <sup>2</sup>
1	Husband's father's to husband's first occupation	.96	.13	-.25	.00	.00	-.01	.02	.03	.02	.99
2	Wife's father's to husband's first occupation	.29	.84	-.36	.00	.00	.00	.02	.05	.01	.92
3	Husband's father's to husband's current occupation	.80	.22	.46	.08	-.03	-.01	.02	.05	-.03	.91
4	Wife's father's to husband's current occupation	.03	.94	.24	.06	.01	-.03	.03	.05	.01	.96
5	Husband's first to his current occupation	-.14	.01	.69	.05	.00	-.03	.04	.02	.02	.50
6	Number of job changes	-.11	-.01	.08	.10	.33	.06	-.02	-.18	.01	.17
7	Income trend in past year	.05	.02	-.06	.28	-.08	-.05	.06	.15	.14	.14
8	Change in husband's earnings	.06	.03	.03	.82	-.02	-.01	.01	.08	.00	.68
9	Change in family income	.01	.04	.01	.85	.11	-.01	.03	.08	.02	.75
10	Number of moves since marriage	-.03	.01	-.01	.19	.70	.09	.08	.09	-.03	.54
11	Length of residence in current place	-.02	.02	.02	.08	-.42	-.05	-.06	-.16	-.16	.24
12	Wife's drive to get ahead	.03	.04	.04	-.01	.07	.09	.87	.03	.00	.77
13	Wife's sacrifice of ideological convictions	-.10	.02	.02	.02	.04	.12	.53	.06	-.22	.37
14	Wife's sacrifice of social interests	.05	.06	-.04	.03	-.09	.02	.64	-.03	.14	.44
15	Wife's social status aspirations	.05	.09	.08	.03	-.07	.14	.10	.12	.11	.08
16	Wife's aspirations for children's education	.08	.05	.06	.07	.00	.01	.11	.14	.26	.11
17	Wife's perception of husband's opportunity	.07	.11	.09	.11	.03	.10	.02	.49	.37	.42
18	Husband's drive to get ahead	-.01	-.03	-.04	-.01	.06	.78	.12	-.02	.04	.63
19	Husband's sacrifice of ideological convictions	.00	-.05	.04	.00	.02	.61	.12	.06	-.11	.41
20	Importance husband attaches to getting ahead	-.10	-.02	-.04	-.04	.04	.36	.07	-.16	.15	.19
21	Level of husband's status satisfaction	.09	.09	-.01	.13	.04	-.04	.03	.75	-.08	.61
22	Commitment to work values	.02	.11	.00	.10	.07	.03	.01	.63	.05	.42
Percentage explained of:											
Total variance		8.8	15.0	2.5	11.7	2.1	10.5	17.2	2.6	0.2	70.6
Common-factor variance		12.0	20.4	3.4	15.9	2.8	14.3	23.4	3.6	0.3	96.1

\* Rotated according to the quartimax criterion of simple structure, which maximizes the fourth moment of the distributions of factor loadings.

<sup>28</sup> We wish to acknowledge our appreciation to the School of Education of New York University for defraying the costs of additional data processing in connection with this analysis.

rough counterpart of this dimension for the wife and includes her willingness to sacrifice social interests.

Factors 8 and 9 are the least clear in the matrix. The former suggests a dimension of subjective mobility—the satisfaction with occupational status as perceived by both husband and wife. Factor 9 may well be mostly error, containing as it does a *potpourri* of low and negligible loadings discouraging other speculations. The only apparent element is the anticipation common to the wife's aspirations for children's college education and perception of her husband's job opportunities.

One further comment about the factor structure seems appropriate. Five of the measures yield small communalities (less than .20). The measure of the wife's Social Status Aspirations ( $h^2 = .08$ ) is the worst offender. Presumably this is indicative of either poor measurement or untapped dimensions—a familiar moot point. The fact remains that these small communalities do not in any telling manner affect our basic conclusions.

#### SUMMARY AND CONCLUSIONS

Critical to the long-run development of

theory in any area is the formulation of concepts. In any science, concept formation is guided initially by imaginative speculation, screened in terms of research utility and accordingly fitted into various bodies of substantive knowledge. This paper has sought to advance concept formation in the area of social mobility by approaching the problem empirically, as well as speculatively, and by applying a model for routinizing the process. The major substantive finding emerging from the analysis is that even if the term "social mobility" is restricted to eleven "objective" and an identical number of "subjective" measures, and the focus of observation is confined to the married couple, at least eight, and possibly nine orthogonal dimensions can be isolated from the total array of 22 measures. One important and direct implication of this finding is that one cannot safely infer knowledge of one dimension of mobility from knowledge of another. More specifically, on the basis of our data, it is erroneous to posit interchangeabilities among and between objective and subjective dimensions of movement, husband and wife variables, and intergenerational and intragenerational mobility. In fact, given nine orthogonal dimen-

sions, one might say that we are prohibited from drawing precisely 72 one-step inferences.

The preceding discussion in no way implies that the concept of social mobility will always be adequately represented by the particular dimensions isolated in this study. On the contrary, there is quite clearly an unavoidable arbitrary element in the specification of dimensions of movement varying with the purposes of the investigation, the unit of observation, the reference points chosen, and the

extensiveness of measurement. For example, in this study, if the requisite data had been available there is every reason to believe that in addition to an *intragenerational* income change factor a comparable *intergenerational* factor would have emerged.

In the light of these considerations, the most general implication of this paper is that "social mobility" is a complex multidimensional concept consisting presently of an indeterminate but substantial number of components.

## GEOGRAPHIC MOBILITY AND EXTENDED FAMILY COHESION \*

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*The hypothesis is advanced that extended family relations can be maintained in an industrial, bureaucratized society despite differential rates of geographical mobility. This is so because institutional pressures force the extended family to legitimize geographical mobility, because technological improvements in communication systems have minimized the socially disruptive forces of geographical distance, and because an extended family can provide important aid to nuclear families without interfering with the occupational system. In support of these views, data are presented from a survey of 920 wives in the Buffalo urban area.*

THIS is the second of two companion papers, both of which seek to demonstrate that *modified* extended family relations are consistent with democratic industrial society.<sup>1</sup> These papers, then, attempt to modify Parson's hypothesis that the isolated nuclear family is the only type which is functional for such a society.<sup>2</sup> Because Parsons so clearly relates his hypothesis to a more general theory of class and business organization there is considerable value in keeping his point of view in the forefront

of discussion, for its modification under such circumstances provides rich intellectual dividends.

Parsons assumes only one kind of extended family relational pattern, the "classical" type exemplified in the Polish and Irish peasant families.<sup>3</sup> There is some evidence, however, for the existence of a modified<sup>4</sup> extended family that is theoretically more relevant and empirically more predictive than either of the two alternatives posed by Parsons' hypothesis—the isolated nuclear family and

\* The author wishes to express his thanks to Glenn H. Beyer, Director of the Cornell Housing Research Center for permitting use of the data in this study, and to Paul F. Lazarsfeld, Arthur R. Cohen, and Bernard Barber for their helpful comments, although they are not necessarily in agreement with the author's point of view.

<sup>1</sup> The first paper is Eugene Litwak, "Occupational Mobility and Extended Family Cohesion," *American Sociological Review*, 25 (February, 1960), pp. 9-21.

<sup>2</sup> Talcott Parsons, "The Social Structure of the Family," in Ruth N. Ashen, editor, *The Family: Its Function and Destiny*, New York: Harper, 1949, pp. 191-192.

<sup>3</sup> These families were marked by geographical propinquity, occupational integration, strict authority of extended family over nuclear family, and stress on extended rather than nuclear family relations.

<sup>4</sup> The modified extended family differs from past extended families in that it does not require geographical propinquity, occupational nepotism, or integration, and there are no strict authority relations, but equalitarian ones. Family relations differ from those of the isolated nuclear family in that significant aid is provided to nuclear families, although this aid has to do with standard of living (housing, illness, leisure pursuits) rather than occupational appointments or promotions.

the classical extended family.<sup>5</sup> The present inquiry supplements the earlier paper by demonstrating that modified extended family relations can be maintained despite differential geographical mobility. The first part of this paper examines the assumptions underlying Parsons' point of view as well as the modification suggested herein. In the second part empirical evidence is presented to show that extended family identification can be maintained despite geographical mobility.

#### GEOGRAPHICAL MOBILITY AND EXTENDED FAMILY ANOMY

There are at least three arguments which support the view that extended family relations are not consistent with geographical mobility: (1) individuals who are strongly attached to their extended families will be reluctant to move even if better jobs are available elsewhere; (2) it is unlikely that identification with extended family will be retained where only one nuclear family moves while the rest of the extended family remains behind; and (3) it is financially more difficult to move a large family and locate jobs for many individuals simultaneously.

The first and third of these propositions suggest that individuals with extended family ties are unlikely to move. The second proposition suggests that if they do move individuals are unlikely to retain their extended family identification with those who remain behind. These arguments can be buttressed by the more general analysis of Homans, who points out that contact is one of the four major prerequisites for primary group cohesion.<sup>6</sup> Since these are familiar arguments they need not be elaborated.

#### GEOGRAPHICAL MOBILITY AND EXTENDED FAMILY COHESION

In this analysis, major attention is given to propositions which are contrary to those stated above, namely, the following: (1) indi-

viduals who are part of a modified extended family grouping are in a better position to move because the latter legitimizes such moves, and as a consequence provides economic, social, and psychological support; (2) extended family relations can be maintained over great geographical distances because modern advances in communication techniques have minimized the socially disruptive effects of geographic distance; and (3) financial difficulties of moving extended families in a bureaucratic industrialized society are minimized because family coalescence takes place when the family is at its peak earning capacity and when it is least likely to disrupt the industrial organization.

1. *Modified extended families aid geographical mobility.* Implicit in the argument that extended family relations lead to a reluctance to move is the view that extended families cannot legitimize geographical mobility. If it can be demonstrated that in current society the contrary is the case, then it can also be shown that such families have far greater facilities than the isolated nuclear family for encouraging spatial movement.

Past instances of legitimization of such movement by the extended family help to clarify the point. In situations of economic or political catastrophe (the Irish potato famine or the Russian pogroms), the extended family encouraged mobility. Given this type of situation, the extended family had at least two advantages over the isolated nuclear family. First, its greater numbers permitted easier accumulation of capital to finance the trip of any given nuclear family. This led to a push and pull kind of migration, with the migrant sending money to help those who had remained behind. Secondly, because of its close ties and size the extended family had superior lines of communication. Thus the migrant became a communication outpost for those who remained behind, providing information on jobs, housing, local social customs, and language. Those who had migrated earlier also could aid the newcomer at the most difficult point of migration.<sup>7</sup>

In a mature industrial society there is great institutional pressure on the extended family to legitimate differential geographical

<sup>5</sup> The counter hypothesis advanced in this paper is a modification of Parsons' position in that it accepts his analysis that the classical extended family is dysfunctional for contemporary society, but it rejects his view that the isolated nuclear family is the only theoretically meaningful alternative.

<sup>6</sup> George C. Homans, *The Human Group*, New York: Harcourt, Brace, 1950, p. 36.

<sup>7</sup> Of the large literature on this point, see e.g., Walter Firey, *Land Use in Central Boston*, Cambridge: Harvard University Press, 1947, pp. 184-186.



mobility among its nuclear family members. This pressure derives from the fact that the extended family can never fully control the economic destiny of its nuclear sub-parts. Although the extended family provides important aid, the major source of economic support for the nuclear family must come from its own occupational success, which is based much more on merit than nepotism. As a consequence, if the extended family wants to see its member nuclear families become successful, it must accept one of the chief prerequisites to occupational success—geographical mobility.<sup>8</sup>

In other words, it is postulated that a semi-independent relation links the nuclear family to the extended family. Because the extended family cannot offer a complete guarantee of occupational success it legitimates the moves of nuclear family members. On the other hand, receiving as it does significant aid in achieving many of its goals, the nuclear family retains its extended family connections despite geographical distance.

2. *Extended family identification is retained despite breaks in face-to-face contact.* There are two reasons why extended families can provide important supplements to nuclear family goal achievement despite geographical distance and therefore two reasons why extended family identification can be maintained despite breaks in face-to-face contact.<sup>9</sup>

<sup>8</sup> C. Wright Mills, C. Senior, and R. K. Goldsen in the *Puerto Rican Journey*, New York: Harper, 1950, p. 51, provide some indirect evidence on legitimation when they point out that the Puerto Rican migrant rarely moves out of a sense of economic necessity but because of a desire for economic betterment. They also show that these migrants rely on extended family communications before migrating (pp. 53-55). These facts illustrate that for the lowest income strata of migrants there has been a legitimation of geographical mobility for maximizing goals. This would seem to be doubly true of the middle-class migrant since he is economically better off to start with.

<sup>9</sup> In addition to these assumptions, two more general ones should be made. First, it is assumed (in counter-distinction to W. F. Ogburn for example, in "The Changing Functions of the Family," *Selected Studies in Marriage and the Family*, New York: Henry Holt, 1953, pp. 74-75) that extended families have not lost their functions. See Litwak, *op. cit.* Secondly, it is assumed that extensive family activity does not lead to occupational nepotism (*ibid.*); but Parsons' hypothesis states that extended family structures will collapse, or nepotism will destroy the industrial order.

As noted above, the rapid development of techniques of communication has made it relatively easy for family members to keep contact despite great distances. Nor does distance, in a money economy, prevent or seriously hinder such aids to family members as help in times of illness, emergency loans or gifts, home purchase, and the like—all at long range.

3. *Geographical coalescence takes place at peaks of earning power.* Although the extended family encourages mobility when it is occupationally rewarding, it does not do so when such moves no longer bring rewards. Given the character of large-scale organizations, there are regular occasions when geographical mobility is not linked to occupational rewards, for example, when the individual is at the peak of his career. The career in the large organization is one in which the individual moves up until he reaches a position from which he can no longer advance; here he remains until he retires. Careers of bureaucrats are rarely downward. Two aspects of this situation are particularly important in the present context: (1) once a person has advanced as far as he can occupationally his working efficiency is no longer tied to geographic moves; and (2) it is at this point that the nuclear family is in the best economic position to support moves of extended family. At this period of his life, the careerist can seek a position near his extended family if he can find a job which matches his present one. Or he can encourage retired parents to settle near him. In short, it is suggested that when the extended family does coalesce it does not lead to undue financial strain (trying to locate jobs for many people simultaneously), nor is it likely to mean an irrational distribution of labor since it involves either retired people or job exchanges between people on the same occupational level.

#### FINDINGS

In order to test alternative propositions about the relationship between family structure and geographical mobility, data from a survey of 920 white married women living in the Buffalo, New York, urban area were analyzed. The sample is biased in the direction of white, younger, middle-class, native-born individuals and as such is not represen-

tative of the total population.<sup>10</sup> However, the bias is a useful one since this is the very group which should most likely illustrate Parsons' hypothesis.<sup>11</sup> If it can be shown that his hypothesis does not hold for this group, then it is unlikely to hold for any division of the society.

1. *Mobility reduces extended family face-to-face contact.* The common basis for the opposing views—that geographical mobility is or is not antithetical to extended family relations—should be made explicit so that it is not mistaken for the main issue. Both positions are in agreement that geographical mobility generally reduces extended family face-to-face contact. Of the respondents in this study, 52 per cent with relatives living in the city received one or more family visits a week. In contrast, only four per cent of those with no such nearby relatives received visits this frequently.

2. *Breaks in face-to-face contact do not reduce extended family identification.* Central to the argument advanced in this paper is the view that geographical distance between relatives does not necessarily lead to a loss of extended family identification. In order to measure family orientation, all individuals were asked to respond to the following statements: (1) "Generally I like the whole family to spend evenings together." (2) "I want a house where family members can spend time together." (3) "I want a location which would make it easy for relatives to

get together." (4) "I want a house with enough room for our parents to feel free to move in." These items formed a Guttman scale pattern.<sup>12</sup> Individuals who answered items 3 or 4 positively<sup>13</sup> were considered to be oriented toward the extended family. Those who answered items 1 or 2, but not 3 or 4, positively were considered to be nuclear family oriented. Those who answered none<sup>14</sup> of the questions positively were classified as non-family oriented.

In order to measure the effects of distance between relatives on family identification, all respondents were divided into two categories, those who had relatives living in town and those who did not. The data presented in Table 1 indicate that geographical distance does not mean a loss of identity. Those who are geographically distant from their relatives are as likely as those who live nearby to retain their extended family identification (22 and 20 per cent, respectively).

Table 1 very likely underestimates the relationship between mobility and extended family identification, since there may have been many individuals who either moved to the community because their relatives lived there or encouraged relatives to come later. In such cases family identification would have been maintained initially despite geographical distance. To deal with this question, all respondents again were divided, this time between those who spent their first 20

<sup>10</sup> The field study was conducted in the Buffalo area between June and October, 1952. For details of the study and the sampling, see Glenn H. Beyer, Thomas W. Mackesey, and James E. Montgomery, *Houses Are for People: A Study of Home Buyer Motivations*, Ithaca: Cornell University Housing Research Center, 1955. Some special features of the sample should be noted here. The sample cannot be considered to be a random one. Being a study designed to investigate housing, five or six different sampling procedures based on neighborhood and housing design were used. The varied nature of the sample complicates the problem of the appropriate statistical test. Therefore the argument must rest heavily on its theoretical plausibility and its consistency with other relevant studies. However, if the assumptions of a random area sample are made, and the sign and Wilcoxon signed-ranks tests are used, then all major findings are significant at the .05 level and beyond. The signs for these tests were always taken from the most complex table in which the given variables appeared.

<sup>11</sup> Parsons, *op. cit.*, pp. 180-181.

<sup>12</sup> Although these items were dichotomized to form a Guttman scale pattern, it is not argued that they meet all of the requirements for such a scale. See Eugene Litwak, *Primary Group Instruments of Social Control in Industrial Society: The Extended Family and the Neighborhood*, unpublished Ph.D. thesis, Columbia University, 1958, pp. 43-47.

<sup>13</sup> The fact that only four per cent of the population answered item 4 positively means that item 3 defines extended family orientation for most of the population. In this connection, no assumption is made that this operational definition exhausts the meaning of extended family orientation; it is only assumed that it will correlate highly with any other measures of extended family orientation.

<sup>14</sup> Because some people may have interpreted "family" to mean only extended family it is possible that in this non-family oriented group there are some people who are nuclear family oriented. This plus the fact that the items were dichotomized to maximize their scaling properties suggests that little reliance should be placed on the absolute percentage of people exhibiting each value position but only on their differential distribution in various groups.

TABLE 1. GEOGRAPHICAL DISTANCE DOES NOT LEAD TO A LOSS OF EXTENDED FAMILY IDENTIFICATION

	Percentage Extended Family Oriented	Percentage Nuclear Family Oriented	Percentage Non-Family Oriented	Total
Relatives living in town	20	52	28	100 (648) *
Relatives living out of town	22	58	20	100 (272)

\* In this and the following tables the figures in parentheses indicate the population base for a given percentage. For tests of significance in these tables, see Footnote 10.

years in the city under study and those who were raised elsewhere. If the latter are considered to be migrants, it can be seen from Table 2 that the migrants (23 per cent) are more likely than the non-migrants (18 per cent) to be identified with their extended families.

3. *Close identification with extended family does not prevent nuclear families from moving away.* Are people who are close to their extended families likely to leave them in order to advance themselves occupationally? To measure the likelihood of persons moving from the community for occupational reasons, the respondents were asked the following question: "Is there a good chance that your husband might take a job out of town?" Those who answered "yes" were classified as potential migrants. To test the likelihood of leaving their relatives, only respondents with relatives in town were examined. It can be seen from Table 3 that those individuals more closely identified with the extended family also were more likely to leave the city and presumably their nearby relatives (23 and 14 per cent, respectively). The same point can be made for the general population if the figures from Tables 1 and 2 are calculated to show how likely family oriented persons are to be migrants. Table 4 presents results which are consistent with Table 3. People are likely to move, then, even when they are strongly identified with

their families, and once having moved away from them, they are likely to retain their family identity.

4. *Bureaucratic career and extended family mobility.* The analysis is thus far consistent with the view that in modern bureaucratic society extended family relations can retain their viability despite differential rates of geographic mobility. To be fully consistent, however, it should be shown that extended family movement is related to career development in the way anticipated by the foregoing discussion. For it was pointed out that it is only when the individual is on the upswing of his career that mobility will be encouraged, while it will be discouraged when he reaches the peak.

In order to measure career stages individuals were asked: "Within the next ten years, do you expect the head of the household will be making: a. a great deal more than now; b. somewhat more than now; c. same as now; d. other, e.g., retired, don't know, etc." Those who said that they expected to earn "a great deal more" income were assumed to be on the upswing of their careers, those who named "somewhat more" were assumed to be fast approaching the peak, while all others were assumed to have reached the peak or plateau of their careers.<sup>15</sup>

<sup>15</sup> Since 95 per cent of the sample subjects were 45 or younger, and since the study was conducted

TABLE 2. MIGRANTS ARE NOT LESS EXTENDED FAMILY IDENTIFIED THAN NON-MIGRANTS

	Percentage Extended Family Oriented	Percentage Nuclear Family Oriented	Percentage Non-Family Oriented	Total
Spent major part of first 20 years in city	18	51	31	100 (504)
Spent major part of first 20 years out of the city	23	56	21	100 (416)

TABLE 3. STRONG IDENTIFICATION WITH RELATIVES DOES NOT PREVENT PEOPLE FROM TAKING JOBS ELSEWHERE

	Among Those with Relatives in the City the Percentage Saying Good Chance Husband Will Take Job Out of Town
Extended family orientation	23 (128)
Nuclear family orientation	18 (336)
Non-family orientation	14 (184)

Table 5 confirms the view that bureaucratic development is congenial to family movement when people are upwardly mobile: 39 per cent of those on the upswing were migrants, while only 16 per cent of those who had reached their career plateaus were migrants.

Two additional bits of evidence supplement this point. First, if the hypothesis advanced in this paper is correct, the individuals who are both extended family oriented and rising in their careers should be most mobile because they have the advantage of aid from their extended families. Comparatively speaking, extended family identity should not lead to mobility when individuals have reached the career plateau. Table 6 suggests that this is the case. When individuals are moving ahead occupationally, those who are psychologically close to their families are much more mobile than those who dissociate themselves from their families (47 and 22 per cent, respectively, are mobile). In contrast, among people at the career peak, the extended family oriented are no more mobile than the non-family oriented (12 and 11 per cent, respectively).

The second bit of evidence which supports the view that extended family aid encourages mobility on the upswing of the career and discourages it otherwise involves the direction of the move. Individuals who have reached the career plateau *might possibly* still move if such moves meant bringing them closer to their extended family. To investigate this possibility, respondents were asked: "Compared to your last house is your present house closer, the same, or farther away from your family?" Table 7 shows that where individuals are climbing the ladder they are as likely, if not more likely, to move away

during a period of great prosperity, virtually no one said he expected to earn less than now.

TABLE 4. PEOPLE IDENTIFIED WITH EXTENDED FAMILY ARE AS LIKELY OR MORE LIKELY TO BE MIGRANTS THAN OTHERS

	Percentage Raised Out of Town	Percentage Having No Relatives in the City
Extended family oriented	51 (187)	32 (187)
Nuclear family oriented	47 (493)	31 (493)
Non-family oriented	37 (240)	23 (240)

from their relatives when they are identified with their extended families as when they are not (53 per cent as compared to 37 and 48 per cent). However, where individuals have reached the occupational plateau, those who are identified with their extended families are less likely to move away from them (38 per cent as compared to 62 and 53 per cent).

In short, the evidence presented here indicates that the career strongly influences the extent and the direction of geographical mobility in a manner consistent with the view that extended family relations are viable in contemporary bureaucratic society.

5. *Bureaucratic and non-bureaucratic careers.* This index of career, however, does not necessarily imply a *bureaucratic* career. Earlier discussions often assume that careers take place in a bureaucratic context. Therefore, the findings of this study should be further differentiated in terms of bureaucratic and non-bureaucratic occupations. In order to isolate the non-bureaucratic career, working-class persons whose fathers were also from a working-class occupational group were segregated from the rest of the population. Non-manual middle-class and upper-class in-

TABLE 5. THOSE ON THE UPSWING OF THEIR CAREER ARE LIKELY TO BE MIGRANTS

	Within the Next Ten Years	Percentage Without Relatives Living in the City
Uprising of career	Expect to make a great deal more than now	39 (183)
Medium point	Expect to make somewhat more than now	29 (603)
Peak of career or plateau	Expect to make the same or somewhat less than now	16 (134)



TABLE 6. EXTENDED FAMILY IDENTIFICATION IS LIKELY TO ENCOURAGE GEOGRAPHICAL MOBILITY WHEN INDIVIDUALS ARE ON THE UPSWING OF THEIR CAREERS

	Within the Next Ten Years	Percentage Having No Relatives in the City		
		Extended Family Oriented	Nuclear Family Oriented	Non-Family Oriented
Upswing of career	Expect to make a great deal more than now	47 (49) *	40 (107)	22 (27)
Medium point of career	Expect to make somewhat more than now	30 (112)	31 (322)	27 (169)
Peak or plateau of career	Expect to make the same or less than now	12 (26)	22 (63)	11 (45)

\* This cell reads as follows: 47 per cent of the 49 people who are extended family oriented and who expect to make a great deal more in the future have no relatives in the city.

dividuals are more likely to follow bureaucratic careers, involving standard promotional steps associated with geographical mobility.<sup>16</sup>

<sup>16</sup> On the basis of the U. S. Census's occupational categories, the husband and the husband's father were classified into: (1) professional, technical, and kindred, and managers, officials, and proprietors; (2) clerical and kindred workers, and sales workers; or (3) all others except farmers or farm help. Husbands' and husbands' fathers' occupations were cross classified to provide four occupational categories: (1) upper-class husbands whose parents were upper-class; (2) husbands whose parents were from a higher occupational group; (3) husbands whose parents were from a lower occupational group; (4) working-class husbands whose parents were working-class. Two groups were eliminated: all individuals of farm background; and middle-class individuals of middle-class parentage (excluded because of the small number of cases). The stationary upper-class group is considered to approximate most closely the bureaucratic occupations while the stationary manual groups are assumed to be the polar opposite. Here "upper-class" does not refer to an old-line "aristoc-

In contrast, these features do not necessarily mark occupational advancement among manual workers. In this group occupational success may mean the achievement of plant seniority or the opening of a small business.<sup>17</sup>

racy" but to a professional-managerial occupational grouping. By definition, all people in administrative positions in large-scale organizations and professionals are included in the upper-class or upwardly mobile occupational groups. There remains the question of whether or not they constitute a sufficiently large number within the overall classification to give a distinct direction. Gold and Slater in a study based upon a random sample of the Detroit area point out that in the one category roughly similar in age and occupation to the "upper-class" in this investigation, 74 per cent of the individuals were members of a bureaucratic organization. Martin Gold and Carol Slater, "Office, Factory, Store—and Family: A Study of Integration Setting," *American Sociological Review*, 23 (February, 1958), pp. 66, 69.

<sup>17</sup> See, e.g., Seymour Martin Lipset and Reinhard Bendix, "Social Mobility and Occupational Career Patterns," in Bendix and Lipset, editors, *Class, Status and Power*, Glencoe, Ill.: Free Press, 1953, pp. 457-459.

TABLE 7. EXTENDED FAMILY IDENTIFICATION IS LIKELY TO ENCOURAGE MOVES \* AWAY FROM THE EXTENDED FAMILY WHEN PEOPLE ARE ON THE UPSWING OF THEIR CAREERS

	Percentage Whose Last Move Carried Them Farther from Their Families		
	Extended Family Oriented	Nuclear Family Oriented	Non-Family Oriented
Expect to make a great deal more in 10 years	53 (49) **	37 (67)	48 (27)
Expect to make somewhat more	52 (112)	56 (322)	59 (169)
Expect to make the same or less	38 (26)	62 (63)	53 (45)

\* Those with relatives in the city were classified together with those without relatives, since the same statistical pattern occurred in each case.

\*\* This figure reads 53 per cent of 49 people who were extended family oriented and who expected to earn a great deal more in the next ten years moved farther away from their families.

TABLE 8. ONLY AMONG UPPER- AND MIDDLE-CLASS BUREAUCRATIC OCCUPATIONS  
DO CAREER LINES PLAY A ROLE

		Percentage Having No Relatives in the City			
	Within the Next Ten Years	Stationary Upper-Class *	Upwardly Mobile	Downwardly Mobile	Stationary Manual Workers
Upswing of career	Expect to make a great deal more than now	43 (76) **	39 (72)	40 (25)	10 (10)
Medium point of career	Expect to make somewhat more than now	42 (146)	39 (183)	26 (99)	11 (176)
Peak or plateau of career	Expect to make the same or less than now	23 (26)	13 (32)	28 (18)	12 (58)

\* For a definition of occupational classification, see footnote 16.

\*\* This cell should read as follows: 43 per cent of the 76 people who were stationary upper-class and who had high expectations of future economic improvement had no relatives in the city.

In such cases success is negatively related to future geographic mobility. As a consequence, a manual worker who envisions an upswing in his career may encourage family members to settle nearby because future success is closely linked to present location. Thus, it is expected that occupational advance has far different meanings for members of the working class and for the middle- and upper-class persons.

In Table 8 it can be seen that the only instances of upswings in careers leading to geographic mobility occur among members of the upper class (43 per cent of those who are on the upswing have no relatives in the community compared to 23 per cent of those who have achieved a plateau). For members of the stationary working class, occupational advancement is least likely, comparatively speaking, to result in geographical mobility (10 per cent of those on the upswing and 12 per cent of those on the plateau have relatives in the city).

Table 8 more than any other should indicate the limitations of the present hypothesis. The latter cannot claim to explain any major features of current American society but only the behavior of members of that group which is often thought to be prototypical of future American society—those belonging to bureaucratic occupations. It is assumed here that future societies will in fact become increasingly bureaucratized. Since Parsons' analysis is largely concerned with this same group,<sup>18</sup> it is maintained that this study

provides evidence contrary to his hypothesis.

6. *The extended family and emotional, social, and economic aid.* Extended families have a unique function in providing aid to those who are moving. This is based partly on the fact that family membership is defined in terms of blood ties and therefore is least pervious to changes in social contact, and partly on the fact that the individual receives his earliest and crucial socialization with people who eventually become extended family members. The individual might find voluntary associations of lesser help than family aid because new personal contacts must be established when one moves, and old contacts tend to have no continuing meaning when geographical contact is broken. Aid from neighbors has somewhat the same character. This point emerges clearly when newcomers to a neighborhood are compared with long-term residents in terms of the average amount of social participation in various areas of life. Table 9 shows that family contacts are as likely, if not more likely, to occur among newcomers than among long-term residents. In contrast, neighborhood and club affiliations are likely to increase the longer individuals live in the neighborhood.<sup>19</sup> This suggests the unique function of the extended family during the moving crisis.

<sup>19</sup> The striking differences between respondents with relatives living in the city and those without nearby relatives, shown in Table 9, are discussed in an unpublished paper, Eugene Litwak, "Voluntary Associations and Primary Group Development in Industrial Society."

<sup>18</sup> Parsons, *op. cit.*, pp. 180-181.

TABLE 9. THE EXTENDED FAMILY MEETS THE NEEDS OF RECENT MOVERS \*

	Percentage Receiving Frequent Family Visits <sup>b</sup>	Percentage Belonging to More Than One Club <sup>c</sup>	Percentage Knowing Five or More Neighbors <sup>d</sup>	Total Population
Respondents Having No Relatives in the City				
Newcomers	22	25	38	110
Long-term residents	16	51	63	166
Difference	08	-26	-25	
Respondents Having Relatives in the City				
Newcomers	54	44	41	163
Long-term residents	49	43	60	485
Difference	05	01	-19	

\* The respondents were divided between the newcomers or those people who had lived in their houses nine months or less and the long-term residents or all others.

<sup>b</sup> When no relatives in the city a frequent visit is defined as one or more family visits a month—either invited or non-invited. When relatives live in the city a frequent visit is defined as one or more family visits a week.

<sup>c</sup> This is the closest approximation to the average number of clubs to which the population belonged.

<sup>d</sup> This is the closest approximation to the average number of neighbors the respondents knew well enough to call on.

#### SECONDARY EVIDENCE

The evidence presented above consistently documents the position that extended family relations are not antithetical to geographical mobility in bureaucratic industrialized society. In fact, at times such relationships actually encourage mobility. The limits of the sample, however, place severe restrictions on the general application of these data. It is of some importance, therefore, to seek in other researches supportive evidence for extended family viability.

First, as a necessary but not sufficient condition, it should be shown that extended family relations are fairly extensive in American society today. In recent years, four studies that provide data on extended family visiting have been carried out, respectively, in Los Angeles, Detroit, San Francisco, and Buffalo. Three of these indicate that close to 50 per cent of the residents made one or more such visits a week. And three of the four investigations, on the basis of comparisons of family, neighbors, friends, and voluntary associations, conclude that the family relationships were either the most frequent or the most vital. These findings, as limited

as they are, strongly suggest that extended family relations are extensive.<sup>20</sup>

What is of even greater interest is that these studies indicate that middle-class white persons share this viability with others and that these relations are highly important ones. Thus Sussman, in a study of middle-class white Protestant families, shows that 80 per cent of the family relationships studied involved giving aid, and in 70 per cent of the cases respondents felt that the recipients would suffer loss of status if the aid were not continued. Moreover, this aid had much more to do with standard of living than with locating jobs or helping people to advance in them through nepotism.<sup>21</sup> This investigation

<sup>20</sup> Morris Alexrod, "Urban Structure and Social Participation," *American Sociological Review*, 21 (February, 1956), pp. 13-18; Wendell Bell and Marion D. Boat, "Urban Neighborhoods and Informal Social Relations," *American Journal of Sociology*, 62 (January, 1957), pp. 391-398; Scott Greer, "Urbanism Reconsidered," *American Sociological Review*, 21 (February, 1956), p. 22; Litwak, *Primary Group Instruments* . . . , *op. cit.*, p. 82.

<sup>21</sup> Marvin B. Sussman, "The Help Pattern in Middle Class Family," *American Sociological Review*, 18 (February, 1953), pp. 22-28 *passim*.

was supplemented by a study by Bell and Boat which indicates that 76 per cent of the low income and 84 per cent of the high income subjects could count on extended family aid in cases of illness lasting a month or longer; they also report that 90 per cent of the respondents indicated that at least one member of the extended family was a close friend.<sup>22</sup> Studies on working class families,<sup>23</sup> Puerto Rican families,<sup>24</sup> Negro families,<sup>25</sup> and Italian families<sup>26</sup> indicate that extended family relations in these cases are viable and warm.

Although these relations are of a far different character from the middle-class family contacts discussed in this paper,<sup>27</sup> the studies of working-class and ethnic groups do provide insight into the extension and warmth of extended family relations in all strata of contemporary society. They do not by themselves refute Parsons' formulation because he assumes that extended family relations are declining, not that they have disappeared. However, they buttress the alternative hypothesis advanced here since they do suggest a basic prerequisite of that hypothesis, namely, that extended family relations are viable in contemporary urban society.

#### CONCLUSIONS

It is argued, then, that these relations can retain their social significance under industrial bureaucratic pressures for geographical mobility. Evidence has been presented that is inconsistent with Parsons' hypothesis. Two theoretical points support this contrary view: first, that the extended family relationship which does not demand geographical proximity (not examined by Parsons) is a sig-

nificant form of social behavior; second, that theoretically the most efficient organization combines the ability of large-scale bureaucracy to handle uniform situations with the primary group's ability to deal with idiosyncratic situations. These two theoretical points suggest that there is both a need and a capacity for extended families to exist in modern society.

The data presented here (and in the earlier companion paper) demonstrate that persons separated from their families retained their extended family orientation; those with close family identification were as likely, if not more likely, to leave their family for occupational reasons; those on the upswing of their careers were apt to move away from their families and to receive family support; those on the career plateau were not likely to move or to move toward their family; that considerations of this kind hold only for bureaucratic occupations; and that the modified extended family seems to be uniquely suited to provide succor during periods of movement. These findings suggest interesting questions for future research. With respect to the family system, there is a need to isolate the mechanisms by which the nuclear family retains its semi-independence while receiving aid from the extended family.<sup>28</sup> It is also important to specify in greater detail the limits of the modified extended family organization in terms of time (does it extend over two or three generations?) and social distance (is it limited, for example, to parents and married children or siblings?). Concerning the occupational system, it is important to identify the type of bureaucratic structure which permits the family to be linked with occupations without affecting productivity.<sup>29</sup> For the analysis of class structure, the question arises as to how likely it is that extended family relations become significant factors blurring class identification without reducing occupational mobility.

<sup>22</sup> Bell and Boat, *op. cit.*, p. 396.

<sup>23</sup> Michael Young and Peter Willmott, *Family and Kinship in East London*, London: Routledge and Kegan Paul, 1957, pp. 159-166.

<sup>24</sup> Mills, Senior, and Goldsen, *op. cit.*, pp. 115, 117.

<sup>25</sup> E. Franklin Frazier, "The Impact of Urban Civilization Upon Negro Family Life," P. K. Hatt and A. S. Reiss, Jr., editors, *Cities and Societies: The Revised Reader in Urban Sociology*, Glencoe, Ill.: Free Press, 1957, pp. 495-496.

<sup>26</sup> Firey, *op. cit.*, pp. 184-186.

<sup>27</sup> Cf. Litwak, "Occupational Mobility and Extended Family Cohesion," *op. cit.*

<sup>28</sup> Cf. Eugene Litwak, "The Use of Extended Family Groups in the Achievement of Social Goals: Some Policy Implications," *Social Problems*, forthcoming.

<sup>29</sup> Cf. Litwak, "Occupational Mobility and Extended Family Cohesion," *op. cit.*; and *Primary Group Instruments . . .*, *op. cit.*, pp. 6-30.



## RESEARCH REPORTS AND NOTES

### A METHOD FOR RANKING COMMUNITY INFLUENTIALS \*

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One of the most popular methods for the study of community influence systems, or power structures, appears to be the utilization, in one form or another, of the "reputational" approach. While studies vary somewhat in specific application of this approach, its main characteristic consists essentially of asking certain people to "nominate" or "vote for" the persons whom they consider to be the most influential in the community. Some fundamental procedures have come to be fairly standardized,<sup>1</sup> typically consisting of the following general phases: (1) A preliminary master list of names of community leaders, or possible influentials, is compiled. These names are derived in a variety of ways, including nominations by heads of formal organizations in the community or by so-called knowledgeable persons (individuals who are presumed to be "in the know" about community power dynamics), information obtained from newspapers, and the investigators' personal judgments. (2) This list of names is submitted to a special panel of "knowledgeables" who are asked to select or vote for those whom they consider to be most influential. (3) Persons re-

ceiving the "most" votes by some arbitrary standard are selected from this list and treated as the influentials of the community. (4) Often an attempt is made to determine a special group of "top" influentials, or "elites," by asking the previously designated influentials who *they* think are most influential, and by additional techniques.

This approach has considerable appeal. It appears to be simple and straightforward. It has the advantage of easy applicability (to local communities of any size). Moreover, it has "face validity"; it obviously "gets at" some aspects of the community influence system. Indeed, the appearance of most of the names at the top of the list will very likely be substantiated by the common-sense judgments of the investigators, or by those of the casual observer of community affairs.

On the other hand, simple as this technique appears to be, it raises some important methodological issues. In effect, this technique is a special type of attitude or opinion polling, and as such it involves the same issues as inhere in the measurement, or scaling, of attitudes and opinions in general. To be sure, this type of scale is not typical of most attitude scales where the interest lies in ranking respondents on one dimension; it is more on the order of what has been referred to as an "object" scale, which is to rank certain objects on the basis of the judgments of some respondents.<sup>2</sup> In either case, there are involved the problems of unidimensionality, the assigning of weights, stability of rank, and the sampling of "items" (objects) and respondents.

More specifically: Is the variable "influence" unidimensional? Does it have the same meaning for all respondents? If so, is the weighting system used appropriate to yield a meaningful rank order? What about the selection of the panel—can it be assumed that they constitute a random sample of a "universe" of panel members? To what extent is the resulting rank order independent of the particular make-up of the panel? Such questions refer to the internal structure or "validity" of the scale.

<sup>2</sup> For a definition and discussion of object scales, see M. W. Riley, J. W. Riley, Jr., and J. Toby, *Sociological Studies in Scale Analysis*, New Brunswick, N. J.: Rutgers University Press, 1954.

\* Data upon which this study is based were collected as part of a study on "Anglo-Latino Relations in Hospitals and Communities" sponsored by the Area Research Center, Michigan State University, under the general direction of Charles P. Loomis. Financial support was received from the National Institutes of Health (Public Health Service) of the U. S. Department of Health, Education, and Welfare and from the Carnegie Corporation.

<sup>1</sup> See, e.g., Floyd Hunter, *Community Power Structure*, Chapel Hill: University of North Carolina Press, 1954; Robert O. Schulze and Leonard U. Blumberg, "The Determination of Local Power Elites," *American Journal of Sociology*, 58 (November, 1957), pp. 290-296; Delbert C. Miller, "Industry and Community Power Structure: A Comparative Study of an American and an English City," *American Sociological Review*, 23 (February, 1958), pp. 9-15; Ernest A. T. Barth and Baha Abu-Laban, "Power Structure and the Negro Sub-Community," *American Sociological Review*, 24 (February, 1959), pp. 69-76.

In this connection, Guttman has emphasized the need to distinguish between two kinds of validity—internal and external.<sup>3</sup> External validity involves problems of prediction (correlation). It is concerned with the question: How well does the scale predict an outside variable (or several variables)? With respect to a scale of community influence, external validity raises several questions: What is the correlation between scale rank and the amount of power or influence a person actually exerts in deciding a real community issue? What is the correlation between influence based upon reputation and formal positions of power? What kinds of power do the individuals have? Does the scale correlate better with regard to certain types of community activity than others? Questions of this nature have been discussed elsewhere.<sup>4</sup> But here we are interested in matters of internal validity.

Internal validity is concerned with scale content, or structure—essentially with the *meaningful ranking of objects along a single attitude dimension*. As Guttman has pointed out, logically, internal validity must be determined *before* external validities can be studied. Of course, the basic function of Guttman scaling theory and technique is to test for such unidimensional ranking.

It is the main purpose of this paper, then, to suggest that Guttman scaling procedures may be used to deal with the problems mentioned above, and that a slight modification of the typical research design permits their application. The following discussion illustrates the use of this approach by reporting the results of its application to a rather typical set of data.

#### PROCEDURE

The data refer to a city in the Southwest with a population currently estimated to be near 700,000. A list of 97 names was drawn in a manner not greatly different from that depicted above. This list, however, is perhaps somewhat untypical in that a deliberate attempt was made to include names of persons from all of the major organizations in the community even

though, in some cases, there was little evidence or expectation that they would be especially influential in the city as a whole. Also, since other aspects of the study dealt with community health problems, there was a deliberate over-selection of names from health and welfare agencies. (One aim was to discover which, if any, of these persons are associated with the upper brackets of the local hierarchy).

This list was submitted to a panel of eight persons. The panel was fairly homogeneous occupationally, all members being involved in the "legal" phases of community affairs. Included were the district attorney, two judges of criminal courts, and several criminal lawyers and an investigator associated with the district attorney's office. The criminal investigator was the only individual without a law degree, but he had held his position for a long time. These panelists were considered to be in a strategic position to observe some important aspects, at least, of the community leadership system. The nature of their official duties, as well as their opportunities for personal contacts with community influentials, presumably gave them considerable "behind the scenes" information concerning the power structure.

Each panelist was given an alphabetized list of the 97 names, with instructions to evaluate each person named with respect to his "general, over-all importance or influence in shaping or determining the affairs of [name of city]." Rating categories included "Exceptionally," "Very," "Fairly," and "Not" important or influential, plus a category for those who felt that they had insufficient knowledge in a given case to express an opinion. In using the Guttman Cornell Technique of scaling, the last category was regarded as the extreme end of the continuum in the direction of "not influential or important." This was justified on the assumption that if a panel member knew little or nothing about a person then the latter could not conceivably have a high reputation as an influential in the mind of that particular judge, notwithstanding any degree of influence he might be accorded by other raters.

In the scaling process, each judge's responses were dichotomized. The criterion for establishing these cutting points was that of maximizing reproducibility. No attempt was made to combine categories arbitrarily so as to achieve certain marginal distributions which are ordinarily considered desirable for scaling purposes. Nevertheless, while the cutting points were not drawn in a *a priori* fashion and no attempt was made to make them uniform for all judges, the dichotomies turned out to be quite similar: in six out of eight cases the cutting point fell between "Very" and "Fairly," in one case between "Ex-

<sup>3</sup> In S. A. Stouffer et. al., *Studies in Social Psychology in World War II, Volume IV: Measurement and Prediction*, Princeton: Princeton University Press, 1950, pp. 57-59. This volume contains the fundamentals of Guttman scaling theory and techniques referred to throughout this paper.

<sup>4</sup> See, e.g., Herbert Kaufman and Victor Jones, "The Mystery of Power," *Public Administration Review*, 14 (Summer, 1954), pp. 205-212; and Nelson W. Polsby, "The Sociology of Community Power Structure: A Reassessment," *Social Forces*, 37 (March, 1959), pp. 232-236.

ceptionally" and "Very," and in another between "Fairly" and "Not" important or influential.

The matter of combining categories in Guttman scaling is somewhat controversial. Some students take issue with Guttman's procedure of arbitrarily combining categories so as to "improve" an item's reproducibility. It is contended that the dichotomizations should be uniform for all items (if a standardized format is employed), or that they should be based on some "logical" decision in accordance with the "meanings" of the response categories (as interpreted by the investigator), or both. For example, in the case of the common five-category scale ranging from "strongly agree" to "strongly disagree," some investigators object to combining a "disagree" with a "strongly agree" response, arguing that the former logically can be combined only with "strongly disagree."

These strictures seem to be a departure from the general scalogram principle of permitting the respondents' over-all response patterns to determine the scale pattern; and such procedures appear to make less allowance for variations in the respondents' verbal habits or mental sets. That there can be wide variations of this nature is well demonstrated by the data of this study. For example, whereas one judge rated only seven persons as "exceptionally" important or influential, another placed 78 persons in that category. Moreover, the latter panelist utilized only the two most extreme categories; he regarded all persons on the list as "exceptionally" influential, except for 19 cases for which he disclaimed sufficient information to venture an opinion.

In any event, in this study the over-all response patterns of the panel members were sufficiently varied to yield satisfactory marginal distributions without extreme, artificial manipulation of cutting points.

#### FINDINGS

The results of the scale analysis are summarized in Table 1. In general, the basic criteria for scalability were met satisfactorily, and the hypothesis of unidimensionality of the variable, influence, was accepted. This is to say that there was good evidence that the eight judges had closely similar frames of reference in their evaluation of the influentials (in relation to the manner in which the instructions were worded), and that the objects could be meaningfully ranked along a single dimension.

However, it will be noted that there were 60 nonscale type responses, and only 38 perfect scale types. The presence of nonscale types has always been problematic in Guttman scaling. Since perfect scales are rarely found in practice, the hypothesis of unidimensionality is usually considered acceptable when the coefficient of reproducibility is around 90 per cent, even though numerous nonscale types may exist. While the presence of a large number of nonscale types does not invalidate the hypothesis of unidimensionality, it does present a problem in achieving a stable, meaningful placement of persons within the scale.

In this study the standard procedure was followed whereby nonscale cases were placed with those perfect scale types which would minimize scale error. In instances where a case would fit equally well in more than one scale type, it was moved as near the middle of the scale as possible.

This procedure is generally conceded to be crude and unsatisfactory, and in the present circumstances it was regarded as especially undesirable. In some uses of Guttman scales, the precise placement of each individual in the scale hierarchy may not be as crucial inasmuch

TABLE 1. SCALE ANALYSIS OF INFLUENTIALS

Scale Type	Response Pattern of Eight Judges*	Perfect Scale Types	Non-scale Types	Total	Number of Errors
8	xxxxxxxx	9	4	13	5
7	-xxxxxxxx	4	1	5	1
6	--xxxxxxxx	1	5	6	8
5	---xxxxxx	1	17	18	27
4	----xxxxx	3	8	11	14
3	-----xxx	2	11	13	15
2	-----xx	2	7	9	7
1	-----x	8	5	13	6
0	-----	7	2	9	4
Total		38	60	97	87

Coefficient of Reproducibility = .89

\* Only perfect scale types are shown. Responses for all judges were dichotomized.

as several scale types may be grouped together later for correlation purposes. But here, where there is apt to be a special interest in the hierarchical placement of each case, the use of such a general rule of thumb was considered to be particularly inadequate. Accordingly, the scaling process was carried further to include the application of Guttman's Israel Gamma technique.<sup>5</sup>

This technique is one of several versions of what Guttman has called the "image" approach.<sup>6</sup> This approach, according to Guttman, "provides a general solution to the problem of *quasi scales*" because the ideal, or image, types tend to converge to form a *perfect scale*. As such, this scale model also provides specific rules for handling nonscale cases; the placement of these cases in the hierarchy now has some logical, mathematical basis. It represents a model for eliminating or reducing extraneous elements

It should be emphasized that people's responses are not "really" changed or manipulated in this procedure; rather, this is an artifact which, theoretically, effects a ranking along the common aspects of the scale and which filters out the idiosyncratic elements. As Riley and her colleagues have put it, "Image analysis appears to stem from the idea that, in a scalable universe, all of the items are related to the basic continuum, and the responses on each item should thus be a function of the responses on all the other items in the scale—except for each item's idiosyncratic component."<sup>8</sup>

Table 3 summarizes the Gamma scaling results. While these did not produce a perfect scale, the procedure served to "improve" the coefficient of reproducibility to 99 per cent,<sup>9</sup> and reduced the nonscale cases to nine. The convergence to a near perfect scale further supports the hypothesis of unidimensionality and,

TABLE 2. EXAMPLE OF IMAGE ANALYSIS FOR ONE JUDGE

Score Received from Judge "A"	Total Score* of Influentials									
	0	1	2	3	4	5	6	7	8	
0		7	14	13	7	5	3	2	2	(53)
1			0	1	7	11	5	4	7	(44)
				0	0	0	1	1**	1**	(97)
									1	1

\* Original responses dichotomized and weighted 1 and 0, for all judges.

\*\* Ties and virtual ties resolved in favor of scale pattern.

from a scale, and hence serves to eliminate or reduce nonscale types by ranking respondents on the *common elements* of the scale.

In an ordinary attitude scale the basic process of the image technique involves a reweighting of an individual's responses to a given item in relation to the manner in which that item was responded to by a majority of those persons who compare favorably with this individual in their responses to all other items. As it was applied here, the technique involved reweighting of the score received by an influential from a given judge in relation to the manner in which he rated the majority of influentials with a similar rating score received from the rest of the judges. This process is illustrated in Table 2.<sup>7</sup>

<sup>5</sup> This procedure is described in Riley et al., *op. cit.*, pp. 390-409.

<sup>6</sup> For a brief treatment of the image approach, as well as another version of it, see Louis Guttman, "The Israel Alpha Technique for Scale Analysis," in Riley et al., pp. 410-415.

<sup>7</sup> A problem arises when the difference between "majority" and "minority" is very small. This is particularly apt to happen in situations like the present one where total frequencies are relatively small. Guttman has suggested the rule that in cases

at the same time, provides a more stable, less ambiguous placement of persons within the scale.

#### SUMMARY AND CONCLUSION

It is the contention of this paper that the use of the reputational approach in the study of

of ties or virtual ties the decision be resolved in favor of the scale type (Guttman, *op. cit.*, p. 414). This resulted in two ties and eight virtual ties (defined as cases where the differences between majority and minority frequencies is not greater than two). In fact, six of these eight cases were resolved in favor of the majority; in only two instances was it necessary to decide in favor of the minority in order to remain in accord with the scale pattern.

<sup>8</sup> Riley et al., *op. cit.*, p. 396.

<sup>9</sup> It should be emphasized that "coefficient of reproducibility" has a different meaning here than the one ordinarily used in scale analysis. As Guttman has stated, "In dealing with quasi-scales, the notion of reproducibility is irrelevant. . . . Instead there are other parameters which should serve as a basis for a theory of reliability of ranks" (Guttman, *op. cit.*, p. 414). Reproducibility as here used is chiefly a measure of the degree to which the image types converge to form a perfect scale.



TABLE 3. ISRAEL GAMMA SCALING OF INFLUENTIALS

Scale Type	Response Pattern of Eight Judges	Perfect Scale Types	Non-scale Types	Total	Number of Errors
8	xxxxxxx	18		18	
7	-xxxxxxx	4		4	
6	--xxxxxx	5			
	x-xxxxxx		1	6	1
5	---xxxxx	20			
	----xx-x		2	22	2
4	-----xxxx	4		4	
3	-----xxx	2			
	-----x-xxx		3	5	3
2	-----xx	7			
	-----x-xx		3	10	3
1	-----x	13		13	
0	-----	15		15	
Total		88	9	97	9

Coefficient of Reproducibility = .99

community influence systems incurs the same basic methodological problems that arise in attitude scaling generally. There is the fundamental problem of obtaining a meaningful rank order of persons along a single dimension; the suggestion here is that this problem may be effectively handled by the use of Guttman type scaling and image analysis. The application of these techniques is demonstrated with one set of data.

It should be remembered that if a unidimensional scale is obtained, it has significance only for that particular situation; since no sampling procedures are ordinarily employed, no assumptions of scalability can be made for another panel of judges or for another community. If panel members are selected purposively rather than on any random basis, they must be regarded as a universe in themselves. The scaling process, then, serves to test for unidimensionality of the scale for that particular collection of judges.

It will be recalled that the panel used in this study was to some extent homogeneous. This may help to account for the scalability; it may explain in part the panel's common frame of reference. However, this type of data would not scale in the *ordinary* sense unless a certain amount of *variation in judgment* expressed by panel members exists as well as a common orientation. To take an extreme hypothetical case in which perfect agreement obtained among the judges on the influence of all persons on the list, the usual parallelogram effect of the scale pattern might not result. If the panel had designated, say, ten persons as "exceptionally" influential, 30 as "very" influential, and the remainder as "not" influential, only three scale types would be possible. Nevertheless, repro-

ducibility would be *perfect*, even though the scale pattern would be irregular.

By focusing attention upon reproducibility as the main criterion of scalability, then, the scalogram can serve as an appropriate model for the data, regardless of the outcome. If there is evidence, as in the example described above, of a shared frame of reference and uniformity of judgments, then the resulting scale pattern, however irregular, would be of special interest and significance in itself.

In practice, however, the problem is more likely to be one of establishing unidimensionality of the variable *influence* than of dealing with uniform responses by judges. Indeed, panels are ordinarily selected with an eye to achieving heterogeneity rather than homogeneity. In such cases the probability of all judges sharing a frame of reference in making their judgments can logically be expected to be reduced, with the result that the final rank order may not be a meaningful one. The methods suggested here are intended to test for such an eventuality.

#### MEN AND WOMEN IN COMMUNITY AGENCIES: A NOTE ON POWER AND PRESTIGE

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This report examines the role played by women in the power structure of a community.

The study focuses on persons affiliated with civic organizations as represented by all board memberships of the Council of Social Agencies in a large northeastern city, as well as the Council itself and the Community Chest. Women participate extensively as members of these boards; of the 1,937 memberships, 522 are held by women. There are 88 agencies affiliated with the Council and 73 of these have board memberships selected on a voluntary basis.<sup>1</sup> These boards are charged with the responsibility for administering a total budget of well over four million dollars. Studies dealing with community power structure indicate a relationship between membership in civic organizations and economic or political dominance or both.<sup>2</sup> Clearly, one way in which women wield power in the community is through membership on boards of social agencies, rather than through association in economic and political structures.<sup>3</sup>

Board memberships have power implications that can be evaluated in a number of ways. The function of the agency is one such criterion. For example, services provided by a hospital are essential to all segments of a community and have considerable potential for control; as an agency the hospital's function is primarily instrumental. Since the board of the hospital has control over it, the board members exercise vital power in the community.<sup>4</sup> On the other

hand, to the extent that the board of a scouting organization controls activities not especially vital to the community, its members wield less power. A further clue to the power component is the control of money indicated by the size of the agency budget. Still another criterion bearing on power is the individual's general status in the community: the higher the prestige, the greater the potential access to board memberships. A related criterion is that of differential status and power based on sex roles.<sup>5</sup> These criteria constitute the basis for analysing the relationship between board membership and power.

Of central concern are the roles played by women on boards of agencies and the relationship of these memberships to the community power structure. Most boards recruit members on a voluntary basis; they can be viewed as voluntary associations. There are 15 agencies however, whose boards are not wholly selected voluntarily; in these cases the boards include paid personnel whose jobs, in part, entail board membership (as in the case of the Sisters who administer a Catholic hospital) or personnel selected through political appointment or public election (as with the Board of Education and the Board of Supervisors of the Department of Social Welfare). In order to insure comparability, only the 73 agencies whose boards are wholly voluntary are included in the present analysis.

It is hypothesized that the more vital the function of an agency to the welfare of a community, the higher will be the rank of the board and the status of its members. Size of budget and the sex composition of the board are additional variables related to the agency function. For example, women are more likely to serve on boards of agencies functioning primarily as expressive organizations than on essentially instrumental agency boards.

#### PROCEDURE

A number of techniques was employed to test the hypothesis. The agencies were classified according to their functions, as instrumental,

Kingsley Davis in *Human Society*, New York: Macmillan, 1949, pp. 94-96.

<sup>1</sup> The 15 boards not included in this study had members who were either political appointees (health officers) or members by virtue of being elected to a public office (on the board of education).

<sup>2</sup> This relationship is especially brought out by Floyd Hunter in *Community Power Structure*, Chapel Hill: University of North Carolina Press, 1953; Robert O. Schulze, "The Role of Economic Dominants in Community Power Structure," *American Sociological Review*, 23 (February, 1958), pp. 3-9; and Robert O. Schulze and Leonard U. Blumberg, "The Determination of Local Power Elites," *American Journal of Sociology*, 63 (November, 1957), pp. 290-296.

<sup>3</sup> Historically, women have been active in philanthropy. That they are playing an increasingly important role in philanthropic activities is suggested by Aileen D. Ross in "Organized Philanthropy in an Urban Community," *Canadian Journal of Economic and Political Science*, 18 (November, 1952), pp. 474-486. A recent report suggests that Negro women occupy an important place in the leadership structure of the Negro sub-community of a large Northwestern city. Negroes, however, were not economically or politically dominant and do not appear to wield significant power in the community. Ernest A. T. Barth and Baha Abu-Laban, "Power Structure and the Negro Sub-community," *American Sociological Review*, 24 (February, 1959), pp. 69-76.

<sup>4</sup> The concept of power used here is defined by

<sup>5</sup> As Parsons suggests, the adult female role continues to be anchored primarily in the internal affairs of the family while the adult male role is primarily anchored in the occupational world. Even though "... it should come about that the average married women had some kind of job, it seems unlikely that such a relative balance would be upset." Talcott Parsons and Robert F. Bales, *Family, Socialization and Interaction Process*, Glencoe, Ill.: Free Press, 1955, pp. 14-15.

instrumental-expressive, or expressive. Instrumental agencies, as a type, may provide a service, produce a product, or serve as organizations designed to maintain or to create some normative condition or change (for example, the Council of Social Agencies or a hospital). Expressive agencies provide a framework within which activity for the participants is immediately gratifying (for example, a settlement house). Instrumental-expressive agencies provide a framework within which both instrumental and expressive activities are self-consciously exercised (for example, the Jewish Home and Infirmary).<sup>6</sup>

The sex composition of the individual boards was determined from a list of all board members. Records pertaining to agency budgets were also utilized. Several persons who were both knowledgeable concerning agencies and boards and active in them were used as judges to rank all of the agencies studied. Included were the presidents of the local Chamber of Commerce and the Council of Social Agencies, a prominent lawyer active in community affairs, two women representing the social elite, and two professional women prominent in community activities. The judges ranked the agencies into four equal divisions, ranging from those which they believed to be most vital to those least vital to the community welfare. In addition, the informants designated the boards on which they would most like to serve. Their board preferences reflected their own evaluations of the agencies' function in serving the needs of the community, their desire to associate with boards whose members were socially prominent, or both.

One criterion employed to determine the status of the individual board members was occupation, classified as follows: director of a large industrial, commercial, or financial enterprise; professional; entrepreneur; and so on. (In the case of women members, their husbands' occupations were used.<sup>7</sup>) Directorships are taken to indicate economic dominance. The second criterion used was membership in the most exclusive private clubs, listing in the social register, or both—as an index of social prominence.

<sup>6</sup> This classification is suggested by C. Wayne Gordon and Nicholas Babchuk in "A Typology of Voluntary Associations," *American Sociological Review*, 24 (February, 1959), pp. 22-29. Were the 73 boards viewed as voluntary associations and placed in the typology, they could be classified as low-accessibility, high-status, instrumental associations. An analysis of the boards as voluntary associations might suggest a basis for further refinement of the above typology.

<sup>7</sup> This is in keeping with the oft-cited principal derivation of status for women and children—noted, for example, by Davis, *op. cit.*, p. 364.

#### INSTRUMENTAL AND EXPRESSIVE AGENCIES

Consistent with the central hypothesis, it was posited that agencies with instrumental functions would not only be more vital to a community but would be more highly valued than expressive agencies. This would be reflected in the relative status of the boards. Furthermore, since men rather than women are economic and political dominants, it was expected that they would dominate the boards of philanthropic agencies with instrumental functions; and that, generally, such agencies would have the largest budgets. On the other hand, it was expected that women more likely would be members of boards of agencies with expressive functions, and with smaller operating budgets.

Of the 73 boards whose membership was voluntary, 40 represented instrumental agencies, 18 instrumental-expressive agencies, and 15 expressive agencies. Eight organizations, including the YMCA, and the Boy Scouts, required that all board members be of the same sex; their exclusion left 38 instrumental, 16 instrumental-expressive, and 11 expressive agencies.

Of the 38 instrumental agencies, 33 were directed by boards having a majority of male membership, of which six were entirely male, 11 more than 80 per cent male, and 11 others between 70 and 79 per cent male. Only five agencies classified as instrumental had boards with a majority of women members: the Society for the Prevention of Cruelty to Children, the City Maternal and Adoption Service, the City Guidance Center (for children), the County Chapter of the Muscular Dystrophy Association, and the Convalescent Hospital for Children. The fact that these agencies provide services for children is consistent with the traditionally defined role expectations for women; they constitute an exception—if numbers are used as a measure of dominance—to the expectation that men will dominate vital instrumental agencies.

Instrumental agencies vary in the degree to which they are vital and controlling. For example, the Community Chest conceivably is more highly ranked than others because it controls funds for all participating agencies, instrumental and expressive. The board of the Community Chest is one of the six whose membership is entirely composed of men. (The other five are boards of the following agencies: three hospitals, a dental dispensary, and the County Medical Society.) Thus, men dominate the boards of most of the instrumental agencies, and the boards of the most vital of such agencies.

Further confirmation for the hypothesis is provided by the board memberships of the 16 instrumental-expressive agencies. Fourteen of

these had a substantial male majority, over 70 per cent in 11 cases. Moreover, the more instrumentally oriented of the instrumental-expressive agencies had a greater proportion of men on their boards (for example, the Salvation Army, 87 per cent; the Jewish Home and Infirmary, 92 per cent.)

More women are found on the boards of expressive agencies, although the board memberships remain predominantly male. The services provided by these agencies relate to the needs of children and young people. The percentages of women on the boards of the three types of agencies are: instrumental 23, instrumental-expressive 32, and expressive 39 per cent.

#### THE RANKING OF BOARDS

As noted above, seven civic leaders ranked the agencies according to how vital they are to the community's welfare. The judges agreed strongly on the ranking of eight agencies considered most vital and eight agencies considered least vital. The "most vital" include five hospitals, the Community Chest, the Council of Social Agencies, and the Visiting Nurse Service of the city and county. Following the ranking, the judges were asked to explicate the criteria they had used, which were named as (a) the agency served a large segment of the community, (b) it controlled considerable funds, and (c) the agency dealt with a crucial problem. The eight agencies (of quite different types) ranked as being least vital include among others, the local USO, the Lutheran Inner Mission, the St. Elizabeth Guild House, and the local Traveler's Aid. These "least vital" agencies serve a limited clientele, have little money, and deal with relatively minor problems.

Of the 222 board members of the agencies ranked as most vital, 198, or 89 per cent, were men. Only 147 of the 240 members, or 61 per cent, of the boards of agencies ranked as least vital were men. Thus, while men tend to dominate the boards of both types, they are more likely to be represented on boards of agencies ranked as most vital. This finding supports the hypothesis that the higher the board's rank, the greater will be the proportion of male members. In addition, all of the agencies ranked as "most vital" are instrumental. The evidence confirms the hypothesis that men are more likely to be on the boards of instrumental agencies, and indirectly provides further support to the proposition that more highly ranked boards are more likely to be predominantly male.

The operating budgets of the eight agencies ranked as most vital totalled 2,916,622 dollars. This figure contrasts with a budget of 276,982 dollars for agencies ranked as least vital. These

budgets include the Community Chest allotments to the agencies for a fiscal year. The difference in Chest allotment to the most vital and least vital agencies reflects a pattern similar to that of the operating budgets. Furthermore, the least vital agencies are among those having the smallest budgets of all organizations affiliated with the Council and Chest.

To test the hypothesis that the higher the rank of the board, the higher will be the status of its members, random samples of 25 persons were selected from the total board memberships of agencies ranked as most vital and least vital. The status of the board members was determined by their degree of economic and social dominance, indicated by occupation (several were directors of industrial or banking enterprises), and by their memberships in the most exclusive clubs or inclusion in the social register or both. Most women on boards derive their status from their husbands or families; exceptions are to be found among professionally trained women. The status characteristics of the members of the boards of the most and the least vital agencies were compared. Eleven of the persons associated with the most vital agencies were listed in *Who's Who in America*; none associated with the least vital agencies was so listed.

Occupations were classified into four categories: (a) directorships—officers of large industries and banks, (b) professionals—limited to physicians, lawyers, and top educators (for example, school superintendent, university president), (c) entrepreneurs of medium sized businesses, and (d) others—including ministers, nurses, engineers, and sales personnel. Eighteen of the 25 board members of the most vital agencies, but only three of the least vital, held directorships, and these 18 held a total of 61 such directorships. Several of these prestigious and powerful persons were associated with each other as directors in the same enterprises.

There were no board members from the most vital agencies in the lowest occupational category (of the four listed above) as compared with 11 board members from the least vital agencies in this category. Thus, persons with high occupational status are more likely to be members of boards which are highly ranked.

The sex composition of the boards ranked as most vital and least vital further differentiates the role of the woman in community power. Of the 25 members of the most vital boards, only one was a woman (a person of social prominence and the wife of the superintendent of schools). In contrast, 13 of the 25 members of boards ranked as least vital were women. Except for two professional women on these boards, the husbands' occupations, used as an



index of economic dominance, were mainly in the lower occupational categories (entrepreneur and others).

Seventeen of the 25 board members of the agencies ranked as most vital, but only 11 of the 25 board members of the least vital agencies, were socially prominent. Of the first 17, 15 were also directors. The 11 board members of the least vital agencies were distributed among all four of the occupational categories. Thus, the hypothesis that the higher the rank of the board the higher the status of its members is supported by interlocking evidence on economic dominance and social prominence.

#### A NOTE ON OCCUPATIONAL ORIGINS OF HEALTH SERVICE PROFESSIONS

DOUGLAS M. MORE

NK and Associates, Inc.

In 1953 Adams reported on the occupational origins of physicians, with special attention to chronological and geographical variations in his sample.<sup>1</sup> The present study does not address these specific questions, but instead applies the method indicated by Adams<sup>2</sup> to two recent cross-sectional samples of students entering medicine and dentistry. It is of interest that there is some continuity with Adams' findings when the method is applied to physicians-in-training born more than a decade later than the youngest men in his sample. The main purpose here, however, is the comparison of dentistry and medicine to test whether or not current populations in these fields are homogeneous in occupational origin.

#### PLAN OF STUDY

There were at hand questionnaire returns from 3,589 entering dental students in the fall of 1958.<sup>3</sup> Of these, 3,355 specified the occupation of the father with sufficient clarity to permit classification. A 10 per cent random sample of "approximately 7,600 freshmen medical students . . . in the fall of 1956" provided secondary data.<sup>4</sup> Of that sample, 673 provided

classifiable information on the occupation of the father. The familiar North-Hatt scale of occupational prestige was applied to each sample.<sup>5</sup> For the medical students, this scale could be used only with reference to grouped data.

In applying the North-Hatt scale to grouped data, an average prestige score for each group must be sought. It seemed that a fairly stringent test for homogeneity of parent populations from which both dental and medical students might have been drawn would be achieved by assigning to the medical student data the same average prestige scores found from a similar grouping of the dental student population. It is possible, however unlikely, that this procedure could have increased the apparent mean prestige score for medical students so much as to create a false difference. Accordingly, the difference between these samples when the data for dental students was grouped so as to correspond to those for medical students was tested.

#### FINDINGS

Table 1 presents the grouped data for medical and dental students, and shows the North-Hatt score assigned to each category. It was possible to assign scores directly from the North-Hatt listings to 2,920 of the 3,355 dental student listings. Interpolated scores were used in only 435 cases. Of course, there was no means of knowing what inherent error may obtain in the medical student sample; it is hoped that this method has not introduced extraordinary bias.

Table 2 summarizes averages and standard deviations for prestige scores of fathers of medical and dental students and the differences between these means with corresponding standard deviations of the mean differences. The differences are listed when the dental student data are both ungrouped and grouped. Even if it is assumed that the mean for the medical students is elevated by grouping in proportion to the apparent elevation of the dental student mean by grouping, all differences between means remain highly significant—they are four to five times as great as the standard deviations of the mean differences found. It is concluded, tentatively, that dental and medical students do

<sup>1</sup> Stuart Adams, "Trends in Occupational Origins of Physicians," *American Sociological Review*, 18 (August, 1953), pp. 404-409.

<sup>2</sup> *Ibid.*, p. 405, note 2.

<sup>3</sup> Douglas M. More, "Social Origins of Future Dentists," *The Midwest Sociologist*, 21 (July, 1959), pp. 70-76. These studies on dental students are made possible by a research grant from The American College of Dentistry.

<sup>4</sup> Helen Hofer Gee and John T. Cowles, editors,

*The Appraisal of Applicants to Medical Schools*, Evanston, Ill.: Association of American Colleges, 1957, esp. Chapter 8.

<sup>5</sup> Cecil C. North and Paul K. Hatt, "Jobs and Occupations: A Popular Evaluation," *Opinion News*, Sept. 1, 1947, pp. 3-13. However, I have used the somewhat more extensive mimeographed lists prepared by Hatt for the research laboratory at Northwestern University.

TABLE 1. OCCUPATIONAL ORIGINS OF DENTAL AND MEDICAL STUDENTS

Father's Occupation	D.D.S. Students		M.D. Students		North-Hatt Score
	No.	Percent <sup>a</sup>	No.	Percent <sup>b</sup>	
Professional	838	23.4	212	28	
(Dentist)	(293)	(8.2)	(15)	(2)	86
(Physician)	(93)	(2.6)	(83)	(11)	93
(Medical related, e.g., veterinarian)	—	—	(7)	(1)	77
(College professor)	(16)	(0.4)	(8)	(1)	89
(Teacher)	(85)	(2.4)	(23)	(3)	79
(Clergyman)	(25)	(0.7)	(23)	(3)	87
(Lawyer)	(41)	(1.1)	(23)	(3)	86
(Engineer)	(103)	(2.9)	(23)	(3)	84
(Other professional)	(182)	(5.1)	(7)	(1)	80
Semi-professional and technical	62	1.7	23	3	73
Managers, Officials, Proprietors	1005	28.1	227	30	75
Clerical workers	111	3.1	15	2	62
Salesmen	319	8.9	53	7	67
Craftsmen	496	13.9	38	5	65
Foremen	80	2.2	15	2	67
Operatives	61	1.7	30	4	55
Laborers	77	2.1	15	2	48
Service workers	104	2.9	15	2	59
Farmers	202	5.7	30	4	75
Total	3355	93.7	673	89	

<sup>a</sup> Per cent of 3,579 cases; 6.3% not classifiable.

<sup>b</sup> Per cent of 756 cases; 11% not classified.

not come from a single population of parents of relative homogeneity as to father's occupation.<sup>6</sup>

#### DISCUSSION

Even though the mean prestige scores for fathers of dental and medical students are fairly close, it is concluded that the occupational prestige of fathers of medical students is on the average significantly higher than that of fathers of dental students. Since the range of scores is

not at issue, it appears that the greater professional "inheritance" for medical students in conjunction with the greater craft "inheritance" for dental students may be producing a very large share of the differences noted. The fact that dentistry places a very high premium on manual skill may in part account for this finding.

For medical students alone it appears that the mean prestige score and its standard deviation have not changed appreciably from earlier decades of this century.<sup>7</sup> Similarly, although only small numbers are involved, it does not seem that the extent of direct "inheritance" into medicine has changed radically during this period.

<sup>6</sup> For appropriate test of homogeneity, see M. G. Kendall, *The Advanced Theory of Statistics*, London: Griffin, 1948, Vol. 1, pp. 226, and comments Vol. 2, pp. 109 (1946).

<sup>7</sup> See Adams, *op. cit.*, p. 406, Table 2.

TABLE 2. RELATIVE PRESTIGE SCORES FOR DENTAL AND MEDICAL STUDENTS

	N	Mean	Standard Deviation	Range
Dental Students				
a. Ungrouped data	3355	72.69	17.38	93-46
b. Grouped data	3355	73.02	9.13	
Medical Students				
Grouped data	673	75.35	10.67	93-48
Mean difference (a)		2.66	~.50	
Mean difference (b)		2.33	~.44	

## COMMUNICATIONS

### ON DURKHEIM: AN EXCHANGE

#### DURKHEIM AND ETHICS

##### To the Editor:

The papers in the August 1959 issue of the *Review* by Robert N. Bellah on "Durkheim and History" and by Harry Alpert on "Durkheim: A Perspective and Appreciation," while greatly clarifying the contributions and perspectives of Emile Durkheim in a great many sociological spheres, tend to present what may be misconstrued as Durkheim's position in the realm of morality and ethics, particularly as concerns the relation between science . . . and religion and morality. . . .

Bellah states that Durkheim "believed that traditional religion was on its way out, *essentially because it conflicts with science*" (p. 460, italics mine). In a similar vein Alpert asserts that Durkheim "sought the foundations for a scientific, rational system of ethics" and that one of the crowning achievements of science "would be the formulation of sound ethical principles" (p. 464).

These statements would appear to bring Durkheim into the camp of scientism, which is completely unwarranted by his own writings. Durkheim explicitly stated in the preface of the first edition of his *Division of Labor* that he had no intention of deriving morality from science but only to make a science of ethics;<sup>1</sup> to study moral phenomena scientifically is quite different from deducing an ethical system from science.

Far from emphasizing a scientific or rationalistic system of ethics, Durkheim always stressed the non-rational bases of religion and morality,<sup>2</sup> such as the significance of faith, not only in traditional religion but even in such modern

secular conceptions as progress and democracy (and even science itself).<sup>3</sup> Collective representations—the cornerstone of Durkheim's sociology—are by their nature non-rational and any morality being a system of collective representations embodied in a social institution is consequently of the same order. If traditional religion or morality has become weakened, it is *not* primarily due to its conflicting with scientific beliefs or to a rational critique of the intellect, but first and foremost because underlying transformations in society have made possible the acceptance of scientific notions by public opinion;<sup>4</sup> intellectual notions such as scientific truths cannot be imposed upon or accepted by an unwilling or unready public opinion (resistance to desegregation in the South amply supports Durkheim's point). Even in the present age characterized by the greater acceptance of scientific truths, Durkheim believed that for a long time mythical truths (*vérités mythologiques*) would also continue to exist in society, and this for a very long time to come.<sup>5</sup>

Finally, Bellah's statement that Durkheim saw the referent of the sacred (or religion) in modern society as coming to be "the nation insofar as it embodies the ideal of humanity" (p. 460) stands in need of correction. The cult of the individual (*not* of the nation), the cult of the human person is the only one destined to survive; it is both the religion and the major if not sole content of our ethical precepts in the present age.<sup>6</sup> To reach this conclusion, Durkheim saw modern society as increasingly characterized by wide differences of interest and background among its members, by the heterogeneity of life in the urban industrial society; the only common denominator between individuals, at least insofar as it provides a basis for a common moral system, is their quality of being human, of belonging to mankind. But, and let us note this well, our notions concerning the sanctity and

<sup>1</sup> "Nous ne voulons pas tirer la morale de la science, mais faire la science de la morale." Reproduced in the 2nd edition, 1902, p. xxvii. Far from undermining traditional morality and faith, the science of moral phenomena would protect them from reason and logical reasoning by showing that the morality we have is the résumé and the conclusion of the history of mankind; this is Durkheim's remarkable position in his article, "La Science Positive de la Morale en Allemagne," *Revue Philosophique*, 24 (1887), p. 284. None of his later writings ever refuted this view.

<sup>2</sup> And more generally, the non-rational elements in all spheres of social life, thereby anticipating Pareto by several years.

<sup>3</sup> "Les Etudes de Science Sociale," *Revue Philosophique*, 22 (1886), p. 68; *Pragmatisme et Sociologie*, Paris: J. Vrin, 1955, p. 184.

<sup>4</sup> Durkheim makes this point in several of his writings, e.g., "Les Etudes de Science Sociale," *op. cit.*, p. 67.

<sup>5</sup> *Pragmatisme et Sociologie*, p. 184.

<sup>6</sup> "L'Individualisme et les Intellectuels," *Revue Bleue*, 10 (1898), p. 11; *Leçons de Sociologie, Physique des Moeurs et du Droit*, Paris: Presses Universitaires de France, 1950, p. 84.

sacredness of the individual (and individual rights) are, for Durkheim, socially given; individualism is a collective representation—perhaps the most important one of modern society.

If I have stressed these aspects of Durkheim's conceptions of moral life it is because of the tremendous significance of that sphere of society in Durkheim's sociology—a sociology which is [fundamentally] a study of moral phenomena as social facts (Durkheim practiced what he taught: his theoretical interest in the division of labor as a key factor in social solidarity is reflected in his own sociological specialization in various facets of moral life<sup>7</sup>). Consequently, misinterpreting him on such an issue as religion and ethics, or the referent of morality in modern society, is to do him a grave injustice.

EDWARD A. TIRYAKIAN

Princeton University

#### THE REFERENT OF THE SACRED? \*

To the Editor:

On the whole I am quite sympathetic to Dr. Tiryakian's views, and the general purport of my concluding remarks was not far from his position. He points out that "Durkheim always stressed the non-rational bases of religion and morality." In the very sentence following the one he quoted from my article I wrote, "But the concept of the sacred would remain: without this basis of moral respect society itself is impossible," which would seem to be saying much the same thing.

On the subject of the referent of the sacred in modern society I do not see a sharp difference in our points of view. The morality which I noted as standing "above both the state and individuals" was indeed a morality in which the "sanctity and sacredness of the individual" (Tiryakian's phrase) was central. I believe I was right in saying this morality stands above individuals for precisely the reason which Tiryakian himself points out, namely that "individualism is a collective representation." I leave it to the reader to decide on the precise balance in Durkheim's mind between the elements of "humanity," "nation" and "individual" after consulting

<sup>7</sup> "Nous ne nous sommes occupé que des règles juridiques ou morales, étudiées soit dans leur devenir et leur genèse . . . soit dans leur fonctionnement . . ." "La Sociologie en France au XIX<sup>e</sup> Siècle (1870-1900)," *Revue Bleue*, 12 (1900), p. 648. This, in Durkheim's own words, is a succinct summary of his main endeavor as a sociologist.

\* Dr. Bellah is not responsible for this awkward title; I am.—*The Editor*.

the references cited in Tiryakian's letter and in my article.

ROBERT N. BELLAH

Harvard University

#### THE FALLACY OF THE MISPLACED QUALIFIER

To the Editor:

Many of us are victims of the fallacy of the misplaced qualifier. We say "natural science" or "social science," but the adjectives are not intended as descriptions of science, but rather: of a particular subject-matter of science. Adjectives, in general, are frightful impediments to clear thinking. Consider, for example, the controversy over Durkheim's conception of sociology as an objective science. On this score, I wrote some years ago: "In the phrase 'objective science' the adjective does not refer to the subject-matter of the science, but to the science itself, that is, to the nature of the body of rules and principles of procedure which we generally call the scientific method. We say that a science is objective when its procedural apparatus includes rules for universal verification and validation, when, in other words, it provides a means by which its empirical results may be accurately and reliably checked by any physically and mentally competent observer who cares to do so. It is only this that Durkheim had in mind when he insisted on an objective method for sociology. Social science, he felt, must meet squarely the apparent paradox of studying objectively subjective phenomena. He always categorically opposed the atomistic behaviorists who, confusing the nature of science with the nature of its subject-matter, discarded subjective data altogether, and thus rejected that which in social facts is social."<sup>1</sup>

Likewise, in a review of a book on foreign policy, I urged the need to distinguish clearly between foreign policy as a social process and foreign policy analysis as a scientific discipline. "Science," I wrote, "refers to a way of understanding phenomena, not to the way in which phenomena behave. Gases do not behave scientifically in following Boyle's Law. Boyle behaved scientifically in developing his law of gases. Similarly, the scientific study of foreign policy does not make the behavior of foreign policy makers scientific. It is illusory to assume that the rational analysis of human behavior necessarily leads people to be more rational in their behavior."<sup>2</sup>

<sup>1</sup> *Emile Durkheim and His Sociology*, New York: Columbia University Press, 1939, p. 112.

<sup>2</sup> *American Sociological Review*, 20 (June, 1955), p. 362.



We must similarly recognize that a science of ethics does not imply that moral behavior is scientific. The rational analysis of the irrational, the objective study of the subjective, are inevitable features of the sciences of human behavior. Professor Tiryakian is correct in identifying Durkheim's concern with the social and non-rational elements in moral and religious behavior.

But why a science of ethics? Durkheim, strongly influenced by Comte, was committed to the positivist doctrine that science can, and should, serve as a guide to social action. (Voir, pour prévoir, afin de pourvoir: to observe, so that one may predict, in order to guide). He firmly believed that the generalizations of a science of moral behavior would provide the foundations for social and moral reconstitution. Durkheim did not hesitate to draw practical implications from his sociological studies, as the justly famous preface to the second edition of *De la division du travail social* makes abundantly clear. In my judgment, Durkheim would have abandoned the sociological enterprise altogether had he not been motivated by the vision of the potential contributions of a science of social facts to human and social betterment. If this be "scientism" let Professor Tiryakian make the most of it!

HARRY ALPERT

University of Oregon

#### ON SOCIAL PROBLEMS AND SOCIAL ACTION

To the Editor:

Your August 1959 issue carried Dr. Mabel A. Elliott's review of our *Social Problems and Social Action*. Dr. Elliott is a distinguished leader of the school of thought which interprets the ills of society as "social disorganization," whereas we prefer the frankly normative "social problems" approach. It was not surprising, therefore, that Dr. Elliott took sharp issue with us on certain points. Quite obviously, she was within her rights in doing so. Indeed, we found her comments very interesting, though they left us still unconvinced.

However, Dr. Elliott went further. She asserted that to one interested in "a scholarly resume of social research" the book would have "little value" and that "theological concepts and doctrines of the church are . . . interwoven with the sociological discussion." In whatever sense Dr. Elliott intended these words, they would sound to the ordinary reader like an accusation that we allowed our ideology to bias our presentation of the scientific facts. This is an accusation which any conscientious social

scientist would resent, and we resent it strongly.

In our book we quite frankly and openly make certain value judgments. For example, we make no secret of the fact that we would like to see eliminated or reduced such phenomena as crime, disease, war, and substandard housing. As the ultimate basis of these value judgments we appealed to Catholic social teaching, but probably few social scientists would disagree with these judgments, whatever their ideological position. We hold that these value judgments are no more unscientific than the medical scientist's judgment that tuberculosis is an evil which should be eliminated.

We used value judgments—and they were frankly based on ideology—to identify what we prefer to call "social problems," but we emphatically deny that we allowed our ideology to bias our description of those problems, their nature, their causes, their extent, and the success of various methods of treating them. Indeed we used much the same literature and came to much the same interpretations as did Dr. Elliott herself in the excellent *Social Disorganization* which she co-authored.

MARY ELIZABETH WALSH

PAUL HANLY FURFEY

The Catholic University of America

#### REPLY TO PROFESSORS WALSH AND FURFEY

To the Editor:

I have re-examined Walsh and Furfey's *Social Problems and Social Action* and can find no essential reasons for altering my conclusions. If my statement that their book is not scholarly is ambiguous, I apologize. The idea I intended to convey was that the book gives only a superficial notion of the large body of scholarly research which sociologists have undertaken in the areas which the book covers. No student will be able to say he is acquainted with the research in the field of social problems from reading this book.

Walsh and Furfey's book, is, of course, not alone in leaning to an over-simplified approach. There has been a rash of textbooks lately which are geared to the lowest level of college students. In this reviewer's opinion it is a great mistake to write college textbooks which over-simplify the basic material. If there is any one thing which is particularly distressing about American college courses it is the trend to the overly simple, non-stimulating material in many colleges' texts.

If the United States expects to achieve any genuine intellectual leadership, college texts and courses should aim at stretching students' minds and not offer them books which they can read

while relaxing from football practice. I am sure Professors Furfey and Walsh are perfectly competent to do a book on a higher intellectual plane. As is, their book is full of pronouncements about natural law being responsible for social problems without any indication of how natural law operates.

The authors deny that their approach is essentially religious, but even so it is full of Catholic theology, and Catholic teaching. Chapter II, "Observe, Judge, Act," for the most part devotes to how a Catholic should proceed with reference to social problems. Research techniques are not developed, however.

Other chapters give considerable space to the Catholic interpretation of the state of the Catholic Church today, the Catholic doctrine on divorce, birth control, the nature of the Christian family, etc. Pope Pius XI is quoted on the maldistribution of income. The Catholic attitude on avarice, papal action in behalf of peace and many other references to the Church's teaching and to papal encyclicals are made.

Natural law itself has been incorporated into Catholic dogma—even though the concept was first developed by the Stoics and expanded by Aristotle. The interpretation of natural law by St. Thomas Aquinas made it an official part of Catholic dogma, as is well known. It is often difficult to apply any theoretical explanation for social phenomena to such diverse items as those covered by *Social Problems and Social Action*, but any over-all explanation should be analyzed and discussed if the explanation is to be meaningful.

As a Protestant who is also a sociologist I, too, have definite religious ideas, but I am still far from convinced that it would be suitable for me to write a college text in sociology interlarded with religious dogma. Where this is done sociology is overlaid with belief and this inevitably limits its scientific objectivity.

This is, to my notion, the dilemma of trying to write a Catholic sociology, an Episcopalian sociology, or a Lutheran sociology. If sociology is true it should not be objectionable to the intelligent Christian of whatever tradition.

MABEL A. ELLIOTT

University of Georgia

#### ON THE STATUS SEEKERS

To the Editor:

William Petersen's "review" of my book *The Status Seekers* [February 1960, pp. 124-126] was not so much a review as a highly emotionalized sputter. The review struck me as a good example of status protection in action by a

member of an in-group. Strike down the impertinent outsider who pretends to have assimilated some of the secret lore of the insiders! Petersen is not prepared to concede that a mere writer who has spent 20 years reporting on studies of human behavior has any "necessary competence to discuss America's social structure."

This attitude apparently relieved him of any feeling that my book needed to be assessed objectively. Instead he merely lists a long series of colorful incidental points mentioned and assumes that the mere listing of them (all torn from context) disposes of me and my book.

At the outset he states that my "principal sources are such dubious ones as Richard Centers and C. Wright Mills." That was news to me (and also seems to indicate Mr. Petersen's own factional viewpoint.) I don't consider those two men as dubious sources, but must clarify that [they] were in no sense my "principal sources." I drew from the studies of more than 150 sociologists. If "principal" ones must be singled out they would be August Hollingshead, Joseph A. Kahl, W. Lloyd Warner, Bernard Barber, E. Digby Baltzell, Milton M. Gordon, and Raymond W. Mack.

I should add that since the book appeared I have heard from a number of the many sociologists I cited. Several have had high praise for the care with which I prepared my book. And of the more than 150 sociologists cited (Petersen was not), not one has complained that I did violence to his particular study in reporting it. I might also add that the book was read in manuscript or galley by six sociologists who made helpful suggestions and volunteered heartening comments. Several commented on the particular care I showed in identifying my own contributions and conclusions as such.

VANCE PACKARD

New Canaan, Connecticut

#### REPLY TO MR. PACKARD

To the Editor:

Mr. Packard finds it difficult to believe that I termed *The Status Seekers* a poor book because I believe it to be poor. My judgment must be based on petty pique, on resentment that he did not include me in his bibliography. He intimates that most who were so honored offered him high praise, and none complained. Can it be that he has overlooked the review in *The Reporter*, by Lipset, who also ranks among Mr. Packard's "principal sources," at least according to the number of citations to his works? By that phrase I meant, of course, those from whom Mr.

Packard had borrowed his orientation, not merely their authority.

A book written for a popular audience must be judged by different standards, even in a review in a professional journal; and it is not easy to state in general terms what those standards should be. Automatic rejection of one who is not in the "in-group," as Mr. Packard puts it and I would not, is never appropriate. In a course I am teaching this semester, the assigned readings include *The Exploding Metropolis*, by the editors of *Fortune*; and I could easily list two dozen other works by journalists or novelists that in my opinion usefully supplement the professional writings of sociologists. Indeed, in the review itself I contrasted Mr. Packard with Samuel Lubell, whose *Future of American Politics* I regard as an important book.

Journalism is a difficult craft, of which in my opinion Mr. Packard is no master. The "colorful incidental points" I quoted were intended to suggest—let me now spell it out—that Mr. Packard has little sense of what is probable and none of what is relevant, that he combines a monumental arrogance with an obsequious deference to authority or money, that his constant effort to vivify his wooden style with the picturesque is not successful. I did not, however, "merely" review these symptoms; two-thirds of the review was devoted to the book's main argument. Since Mr. Packard does not mention these basic criticisms, there is no need to repeat them here.

If *The Status Seekers* is quite so bad, Mr. Packard might well ask, why has it been so great a success? Why has it been on the best-seller list for some forty weeks? The answer to this interesting question is not merely its style, although the author's willingness to simplify all concepts, to interrupt any passage with a more or less pertinent anecdote, certainly contribute to "easy reading." The appeal lies, I believe, in

the social climber's ambivalent stance toward social climbing. In the more naive past, the upwardly mobile exerted a heavy demand for works on "self-improvement"—aids in acquiring the language, manners, symbols of the class into which they were moving. Today, large numbers have advanced far enough to know that they must not admit the need for such assistance, however anxiously they feel it. In his two books, Mr. Packard has applied the perfect formula for this audience, one that had been perfected long since in moral tracts against pornography. It is necessary, one regrets, to describe in copious detail the tricks by which one can achieve new status, and this manual is as useful as an up-to-date Emily Post. Then, having eaten one's cake, one can have it still by denouncing the status-seekers as an unworthy lot (or even a non-existent one; Mr. Packard is competent enough to find variations on the theme.) Such careful catering to a widespread need suggests that the author is a good businessman, but it was not as such that I judged him and his book.

WILLIAM PETERSEN

University of California, Berkeley

#### A CORRECTION

To the Editor:

In Melvin Seeman's article, "On the Meaning of Alienation" [the *Review*, 25 (December, 1959)], there is a minor error. The scales to which he refers (p. 789, n. 20) were developed to measure powerlessness, normlessness, and social isolation. Incidentally, they intercorrelated with each other from .41 to .67 (N 384), thus seeming to support his conceptualization of alienation as having several components.

DWIGHT G. DEAN

Denison University

# THE PROFESSION: REPORTS AND OPINION

## OFFICIAL REPORTS AND PROCEEDINGS

### Minutes of the Meeting of the Executive Committee

March 25, 1960—New York City

Present: Howard Becker, John Clausen, Kingsley Davis, Robert Faris, William Goode, George Homans, Wilbert Moore, Charles Page, John Riley, Irwin Sanders, Donald Young, and Matilda Riley, *ex officio*.

1. *Location of the Association's Offices:* In agreement with the recommendation of the Committee on the Profession, the Committee voted that the central office should remain in New York but that arrangements should be made as early as possible for high-level Washington representation. The Committee expressed warm appreciation to New York University for its hospitality and to the office staff for its competent and loyal services.

2. *Change in Administration:* The resignation of the Executive Officer was accepted with regret, but with gratitude for past services, and with the hope that she will continue with the Association until appropriate, new arrangements are made.

In view of the resignation of the Executive Officer and the expiration of the term of office of the Secretary, a sub-committee (composed of Donald Young, John Riley, and Wilbert Moore, with the assistance of the Executive Officer) was asked to explore the future administrative problems of the Association. This sub-committee is to make recommendations to the Executive Committee and the Council, defining with a degree of flexibility the various roles which may need to be filled.

3. *Index to the Review:* A budget of 350 dollars was set up for clerical assistance and expense for the 1960 revision of the *Index*.

4. *Membership Directory and Address Lists:* Because of the high cost of the address list proposed as an interim supplement to the Directory of Members, it was decided not to issue this list until 1961. A new Directory is to be published in 1963.

5. *Subscriptions to Other Journals:* The Publications Committee was authorized to formulate a definite policy on subscription rates for journals of other publishers, with consideration given to a minimum saving to members (for example, 15 per cent) plus a han-

dling fee for the Association (for example, 10 per cent).

6. *Certification of Social Psychologists:* In accordance with the recommendation of the Committee on the Profession, the Executive Committee asked the President to Communicate with the Presidents and Council Representatives of the Affiliated Societies about developments in regard to the certification of social psychologists. The American Sociological Association has accepted responsibility in this matter, has entered into a basic agreement with the American Psychological Association, as published in the *Review*, 24 (June, 1959), p. 402, is working out details of a plan for certification of sociologically trained social psychologists, and is working on specific legislative problems at both the national and the state levels. The Committee believes that, in the interests of the profession as a whole, it is important that sociologists understand the implications and importance of these agreements. This matter will be discussed by the President at the luncheon for officers of affiliated societies at the time of the annual meetings.

7. *Journal of Educational Sociology:* The Chairman of the Publications Committee was instructed to appoint a committee to investigate the feasibility of obtaining a subsidy for the publication of this Journal.

8. *Russell Sage Bulletins:* Two new bulletins were approved: *Sociology and Law*, to be prepared by William Evan; and *Sociology and Corporate Management*, to be prepared by Raymond Bauer. Committee members were asked to send suggestions for further bulletins directly to Leonard Cottrell, Editor of the series.

9. *The 1960 Criminological Conferences:* It was voted that the Association send one delegate to represent the Association at both conferences, and the following nominations in order of priority were approved: Donald Cressey, Marshall Clinard, Gresham Sykes, and Norman Hayner.

10. *New Sections:* Since numerous policy questions have arisen in regard to Sections of the Association, the Committee on the Profession was asked to study these matters on a continuing basis and to make recommendations to the Council. Two actions were taken:



(1) An Application for a Section on Assumptions, Axioms and Postulates in Sociological Theory was laid on the table pending recommendations from the Committee on the Profession.

(2) The original nominating committee of each new Section is to be appointed by the President, as recommended by the Committee on the Profession, so as to assure representation of the field as a whole as well as of the informal organizing group.

11. *Reballot for Publications Committee*: In view of a typographical error in the recently mailed election materials, the Executive Office was instructed to hold a reballot for the office involved if this becomes necessary.

12. *1960 Conference Committee*: The slate as prepared by Rex Hopper was approved and the Chairman empowered to make additions and changes as needed.

13. *"The American Sociologist"*: The inauguration of this publication was postponed until the new administration of the Association is in office, at which time the issue will be re-examined.

14. *1962 Meetings of the ISA*: It was agreed that Conrad Taeuber should be asked to investigate possibilities of Federal funds in support of these meetings.

15. *Civil Service Series*: In line with the recommendation of the Committee on the Profession, the President was asked to write a strong letter to the U. S. Civil Service Commission in support of a ruling that a social psychologist with a Ph.D. in Sociology be recognized as qualifying for the title of Social Psychologist in the psychology series.

16. *Funds for the Research Committee*: It was voted to appropriate 1,000 dollars to the Research Committee for an evaluation project provided that the balance required by the project would be supplied by one of the foundations. It was further voted that a full report from the Committee be submitted to the Council, which will then decide how much of the report will be published as part of the Official Proceedings.

17. *Representative to the National Association of Social Workers*: The President is authorized to join with the appropriate officer of the NASW in appointing an individual to serve both organizations in a liaison capacity. In the meantime, the Secretary was asked to write an appropriate letter to the NASW endorsing Joseph Eaton as the present incumbent.

18. *The Council for Research in Education*: The question of the Association's possible affiliation with the Council was tabled.

19. *Committee on Professional Ethics*: The Executive Committee has taken note of the

recommendation of the Committee on the Profession and approves its establishment of a new Sub-Committee on Professional Ethics.

Respectfully submitted,  
DONALD YOUNG, *Secretary*

## SOME REFLECTIONS ON BUREAUCRATIC TRENDS IN SOCIOLOGICAL RESEARCH \*

WILSON RECORD

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During the past fifteen years two closely related and highly significant developments have occurred in American sociology. They are the large-scale organization and financing of research and the emergence of "team" or collective-type research endeavors. While these changes have received much informal attention, little had been done toward exploring them publicly or debating within the profession either their short-run or long-term consequences. The following remarks are intended to suggest certain implications of these developments and to pose propositions for further discussion.

It has become commonplace in sociology to observe that the "days of one-man research are over." Usually the statement is made not with a feeling of regret, but in a mood of optimism—or even exhilaration. "Group" or "team" endeavors, carried on within a highly structured institutional setting, more and more come to be the accepted means of formulating theories, testing hypotheses, and furthering knowledge.

Although sociology, until now, has been less affected by this trend than some other social sciences, particularly psychology, craftsmen in our field can hardly be unaware of the intrusion of mass production methods in sociological research. Now it is easier for a "team" of sociologists to secure a grant of 100,000 dollars for a collective project than it is for each member to obtain 10,000 dollars—or even 5,000 dollars—for unrelated individual undertakings. Indeed, the merits of proposed research are judged largely by two criteria: the cost of the undertaking and the number of participants. The assumption seems to be that the greater the amount spent and the larger the number of people involved, the higher the quality of the research effort.

The arguments in favor of this collectivization

\* Several helpful suggestions came from a paper in preparation by my wife, Jane Cassels Record, on the relative vulnerability of research institutions to organized pressure groups.

of inquiry include several considerations: (1) the many facets, each deemed to be of more or less equal relevance, of even the most minute problem; (2) the complexity of data which somehow must be sifted and reduced to specified quantitative dimensions and correlated to establish relationships; (3) the enormous amounts of time and energy needed to secure and process essential material; (4) the elaborate apparatus including space, files, mechanical equipment, and laboratories without which presumably no worthwhile research enterprise could proceed; (5) the limits of the individual investigator's intellectual resources, however well developed those resources may be. The assumption has been that in these circumstances joint inquiries—sponsored, shaped, "coordinated," and administered by an institution headed by people with high organizational skills, if not wide grasp of subject matter—are the appropriate, indeed, the only acceptable modes of inquiry.

In the process, however, the large corporation's monopoly on groupism and "togetherness" has been broken. And "teamism" as an operating principle is no longer the exclusive property of the present national administration. "Organization men" are now found in the most unexpected places, long distances from the caverns of Madison Avenue. More research, of course, has been undertaken, and it is presumed to exhibit a thoroughness, precision, and sophistication beyond the reach of any individual sociologist. Perhaps such organization of inquiry has sharpened our theoretical and methodological tools, enabled us to collect and process a vast amount of heretofore unobtainable data, and in the end, furthered our knowledge of human behavior. But I find it difficult to accept such a conclusion, and I am disturbed by the fact that many of my colleagues seem able to encompass most of this knowledge without much disturbance.

In the enthusiasm of the moment, generated in no small part by the wider availability of funds, if not by the desire to explore challenging ideas, we have ignored a number of liabilities which, I suggest, are endemic to "team" research endeavors. We have silenced our voices, if not closed our eyes, to the limitations imposed on the sociologist by the very institutional structure essential to the collective type of inquiry. These limitations are unavoidable, leaving the conscientious investigator with but two alternatives. Either through a process of self-hypnosis he begins to identify personal, intellectual, and research interests with group well-being and institutional stability or he severs his commitment and resumes his journey along the lonely road of individual endeavor. Admittedly, neither choice is a particularly happy one. But it is high

time we speak out about what is taking place. Perhaps we could begin by taking seriously some of our own observations about the constricting character of institutional demands.

A research organization, like other institutions, acquires a conservatism that in time becomes chronic. Although committed initially to the autonomous and unfettered quest for truth, its members soon discover that choices must be made between that objective and the growth, or at least the continued functioning, of the organization *qua* organization. Especially is this true when the research activity involves examination of other institutions whose members have the will—and the power—to retaliate if objective findings are deemed harmful to them. Other forms of coercion, far less crude than direct pressure, may be and usually are employed. The fact that the research agency's sources for data are other institutions gives the latter a check-rein pulled by the threat to withhold needed information or refuse cooperation. Wanting to stay in business, and not above engaging in what Reisman has termed "academic check-kiting," the research staff cannot afford to jeopardize these sources. In time, the personnel develop a sensitivity about how far they can go and how much they can get.

These processes of internal discipline and external compromise are insidious; the researchers, and even their more knowledgeable superiors, are never sure when the line separating institutional needs from intellectual integrity has been crossed. And, eventually, it becomes much more satisfying not to bother with the question at all. Each challenge can always be compromised on the ground that the isolated instance is relatively unimportant, that it never justifies a potentially fatal institutional crisis. Appropriate rationalizations are always available, and, with experience, those involved can surrender while believing they have stood firm or even won a victory for truth. It may sound trite—but it is true—that the sociologist in such circumstances vacates professional principles not by the mile, but by the inch. But vacate them he does.

In some cases, of course, the capitulation of an agency and its personnel may be sharp and drastic. Probably we all know of instances in which research was completed, given limited circulation, and then withheld following objections from powerful quarters. Without doubt, we are all aware too of qualifying interpretations being issued by a research institute in the wake of criticisms of findings which are made public, and of at least a few cases in which a research group under extreme pressure has gone so far as to disown outright the results of its best efforts. Conceivably, in no instance was any ques-

tion raised concerning the theoretical and methodological soundness of the inquiry.

The research institute, like the individual investigator, to be sure, cannot control the uses or interpretations of its scholarly output. On occasion, its members are painfully surprised at the unanticipated reception of their endeavors. However, from such experience they learn much—what is absolutely safe, what is questionable, what is risky, what is dangerous, what is fatal. But they cannot always be sure into what category a given project will fall, and so they tend to play it safe. Those who fail to learn—and some can't or won't—become organizational liabilities. They must be isolated within the structure, restrained by their superiors, kicked upstairs—or down, or *in extremis*, eliminated outright.

Moreover, the selection of additional or replacement personnel comes to be predicated not solely on talent or demonstrated skill, but on the individual's capacity to subordinate scientific concerns to institutional needs, or at least to pursue the former within an increasingly rigid framework. Eventually such selectivity produces a common outlook and a standard organizational image among the participants. Only those outsiders willing to take the vows of conformity, or whose regularity is so transparent as to require no proof, are admitted to the order. The "enthusiast," as Roche has pointed out, is subordinated to the bureaucrat. The stability of the order rather than realization of the high principles to which it was initially dedicated, indeed, the justification for its establishment, becomes the major concern. Moreover, the organizational researcher tends to lose his capacity for moral indignation, which, I suggest, is a crucial source of insight.

Although these processes apply to research institutions generally, they have a special significance for sociologists. We are not surprised that a trade union, a corporation, a political party, a religious order, or a medical society exhibits such patterns. On the contrary, sociologists, perhaps more than members of any other group, have developed a body of theory and supporting evidence whereby structural and functional aspects of institutions have come to be better understood. I, for one, however, am surprised—and disturbed—by the fact that we have accepted this institutional pattern in research without raising fundamental questions concerning either its moral and intellectual desirability or its efficiency as a means of furthering knowledge. Why have we been so reluctant to apply critically our organizational theories to ourselves and to what may be the most vital of our professional endeavors? Why have we ac-

cepted with such small disclaimers the organization of our intellectual enterprise on the basis of those principles that we often characterize as limiting, stultifying, and destructive of creativity in other institutions?

Obviously, I cannot answer these questions here—and perhaps not at all. But I am convinced that they ought to be raised, and moreover, that the answers will have more than a strictly academic significance. These and related queries will lead to a consideration—perhaps for a few, a reconsideration—of a whole series of issues heretofore brushed aside. Before requesting large grants, joining research "teams," and participating in collectivized inquiry of the type described above, we might consider such propositions as the following and turn our talents, such as they may be, to their exploration:

1. The superior value of "team" as opposed to individual research remains to be demonstrated on its merits, this apart from other dangers and limitations to which I have referred.

2. The "team" type of research frequently forces the sociologist to engage in inquiries not for testing original hypotheses, but to make maximum utilization of research staff and equipment which might otherwise be idle.

3. The processes that operate in the selection of "team" research projects lead to a neglect of relevant problems, not because of theoretical or methodological considerations, but because of institutional needs.

4. The selection of "team" research personnel makes for commonality and "inbreeding" that have serious implications for the advancement of sociological knowledge, a product in part of conflict on the intellectual plane.

5. The "team" approach to research is conducive to intellectual flabbiness since the project comes to represent a compromise of individual ideas and a consensus arrived at under forced draft to participate in the group and to keep the machinery rolling.

6. "Team" research leads to the fragmentation of sociological knowledge since it tends to bring together narrow-gauged, and possibly narrow-minded, specialists, each of whom pursues his particular line without knowing, and perhaps not caring, about the direction in which the whole is moving.

7. A corollary of this proposition is that the individual is discouraged in attempts to formulate comprehensive and integrated views of the discipline; instead, a premium is placed on his becoming an academic assembly-line man.

8. Institutionalized research in this form leads to undue emphasis on quantification in sociology since: (a) Staff and equipment cannot at times be otherwise employed and there is pressure to

justify the continuing overhead. (b) Rigid quantification provides a protective "team" and institutional coating whereby relevant findings are underestimated and technique is overemphasized. (c) There is pressure to duplicate operations of other research groups having similar structures but quite different subject matter which *does* require a high degree of quantification. (d) Quantification may become primarily a public relations and business-drumming device since it can be employed to convince the uninformed, but influential, that research must be significant because it involves mathematical operations. (e) Statistical manipulations can become a substitute for rigorous intellectual endeavors, obscuring simultaneously the fact that thinking in the final analysis takes place in the sociologist's mind, and that he rather than the statistical principles or the machines is responsible for what he does or doesn't do. (Perhaps this is not unrelated to the fact that sociologists no longer write books on broad sociological theory. Instead, we get out collections of "readings in theory," with the editor's interspersing comments that are supposed to make the individual offerings hang together in some coherent whole.)

9. "Team" research is a relatively easy means for escaping individual intellectual responsibility, enabling the researcher to pass the buck to other members who, in turn, can pass it back to him, while the critic can never identify the culprit. Or, it enables the researcher to lodge failure in the group as a whole. In any event, responsibility is hard to pin down.

10. Finally, the whole process leads to a pattern of inquiry which insofar as it is institutionally "safe" may be to that degree sociologically insignificant.

#### THE ROLE OF THE ACADEMIC MAN AS A CROSS-CULTURAL MEDIATOR

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Organizational machinery now exists which enables the academic man to play a unique role in facilitating cultural mediation among nations. Through the Fulbright Program and other arrangements for educational exchange, an unprecedented number of positions for visiting professors, researchers, and students has been established in universities outside of the United States. The present paper discusses several implications of being a temporary envoy and the fact that persons playing this role are members of the academic community. We shall concen-

trate on the Fulbright Senior Scholar as the illustrative case. Ordinarily such a grantee occupies a position as Visiting Professor within a university during his stay abroad.<sup>1</sup>

Expected activities of Visiting Professors in teaching, conducting research, participating in professional discussions and professional meetings require frequent interaction throughout the year with various persons in the host countries. These activities are usually such as to maintain emphasis on a common task, whether it be focusing on the substantive content of a course or seeking the discovery of new knowledge. Further, the tasks occur within a context of values and norms common to the academic communities of many societies throughout the world—for example, dedication to one's profession and maintenance of high standards of scholarship and research. A final element of the position of Visiting Professor is that officially it is of approximately equal status to that of professors in the host institution, yet the visitor is not in direct competition with his hosts, at least not within the same system.

If we view the nations within a social system frame of reference, certain implications for educational exchange follow. One of the characteristics of any social system is the existence of complementarity of expectations. That is, its actors tend to share a common set of value orientations guiding behavior in the various positions within the system. Consequently, when two actors are interacting, each can anticipate the other's responses with sufficient accuracy so that his behavior is likely to bring the results he desires. This, of course, involves the double contingency in role expectations that Parsons describes. When, however, an actor moves from the system in which he is a member into another system where the set of expectations for his position differ, inappropriate role behavior is likely to ensue. For example, while overseas one visiting American Fulbright professor followed his usual practice of going to class meetings early in order to chat informally with students—until he learned that students who arrived after him turned away without entering the classroom. Respect for the teacher decreed that students be in their seats awaiting him when he arrived.

The visiting foreign professor is a stranger in the host academic system in the sense in which

<sup>1</sup> The status of Visiting Professor is presented here as an ideal type. While we are aware of the vicissitudes of enculturation, the problems created by inflexible reference group behavior, and so on, we highlight those aspects of the status which facilitate cross-cultural mediation. Thus, we describe the limiting case of the frictionless Visiting Professor, comparable to the frictionless machine.



Simmel uses the term—the person who comes today and stays tomorrow. “His position in the group is determined, essentially, by the fact that he has not belonged to it from the beginning, that he imports qualities into it, which do not and cannot stem from the group itself.”<sup>2</sup> If the group is to utilize these importations effectively, a status must be provided for the stranger which legitimizes his membership in the system and authenticates his competence. For the Fulbright grantee, the position of Visiting Professor serves this function. It not only legitimizes the man and his communications, but also provides a number of structured situations within which it is expected that those to whom he speaks will listen to what he has to say. It is expected that he in turn will be seeking to learn from his peers in the host institution. Knowledge of the host country’s language, generally a prerequisite for appointment, ensures that he will have at least the basic tool for communicating.

Relations among racial and cultural minority systems, or between a minority and the dominant society, resemble in many ways the relations between national social systems. These are all situations of systemic linkage; thus research in the field of minority groups suggests hypotheses that appear to be applicable in studying the impact of international educational exchange.<sup>3</sup>

Many American studies of the effects of interracial interaction have focused on Negro-white relations in formalized social settings under considerable authoritative control. MacKenzie found that in a governmental organization and in two large universities, association of whites and Negroes of high occupational status led to the breaking down of stereotypes. In the Army, public schools, the Merchant Marine, and in housing projects the degree of interaction between Negroes and whites has been found to correlate with the degree of positive feelings and with the lessening of unwarranted hostile stereotypes.<sup>4</sup>

<sup>2</sup> Kurt H. Wolff, translator and editor, *The Sociology of Georg Simmel*, Glencoe, Ill.: Free Press, 1947, p. 402.

<sup>3</sup> See Robin M. Williams, Jr., “Racial and Cultural Relations,” in J. B. Gittler, editor, *Review of Sociology: Analysis of a Decade*, New York: Wiley, 1957; George E. Simpson and J. Milton Yinger, *Racial and Cultural Minorities*, New York: Harper, 1958, Chapters 3–8, 22, and 23; Edward A. Suchman, John P. Dean, and Robin M. Williams, Jr., *Desegregation: Some Propositions and Research Suggestions*, New York: Anti-Defamation League of B’nai B’rith, 1958. An insightful treatment of systemic linkage is found in Charles P. Loomis, *Social Systems: Organizational Persistence and Change*, Princeton, N. J.: Van Nostrand, forthcoming.

<sup>4</sup> Barbara K. MacKenzie, “The Importance of

Williams has hypothesized that attitudes toward outgroup members “in a situation of outgroup contact are most easily changed *with reference to that type of situation* if the focus of attention is upon a common interest, task, or group goal, without any explicit reference to racial or cultural membership of the participants.”<sup>5</sup> He notes that such contacts should be relatively enduring and warns that positive attitudes generated in these interactions may not characterize other situations or other outgroup members. Lewin and Allport believe that positive attitudes may become more generalized under favorable conditions. Allport adds that such contacts, to be maximally effective, should “occur in ordinary purposeful pursuits, avoid artificiality, and if possible enjoy the sanction of the community in which they occur.”<sup>6</sup>

The situation surrounding the status of Visiting Professor conforms closely with the conditions which have led to the establishment of closer relations between minority groups and the dominant white group in America. Interactions within the university systems occur in a social setting of formalized authoritative control, thus lessening the possibility of early tensions or misunderstandings being overtly expressed and disrupting the relationships. The Visiting Professor occupies a high occupational status and his relations with students and faculty are maintained throughout an academic year. Both lectures and research by the Visiting Professor occur within the normal university

Contact in Determining Attitudes Toward Negroes,” *Journal of Abnormal and Social Psychology*, 43 (October, 1948), pp. 417–441; S. A. Stouffer et al., *The American Soldier: Adjustment During Army Life*, Princeton: Princeton University Press, 1949, Vol. I, Chapter 10; R. M. Williams, Jr., and M. W. Ryan, editors, *Schools in Transition: Community Experiences in Desegregation*, Chapel Hill: University of North Carolina Press, 1954; Ira N. Brophy, “The Luxury of Anti-Negro Prejudice,” *Public Opinion Quarterly*, 9 (Winter, 1945), pp. 456–466; Morton Deutsch and Mary E. Collins, *Interracial Housing: A Psychological Evaluation of a Social Experiment*, Minneapolis: University of Minnesota Press, 1951; Robert K. Merton, “The Social Psychology of Housing,” in W. Dennis, editor, *Current Trends in Social Psychology*, Pittsburgh: University of Pittsburgh Press, 1948; Daniel M. Wilner, Rosabelle P. Walkley, and Stuart W. Cook, *Human Relations in Interracial Housing: A Study of the Contact Hypothesis*, Minneapolis: University of Minnesota Press, 1955; Melvin M. Tumin, *Segregation and Desegregation: A Digest of Recent Research*, New York: Anti-Defamation League of B’nai B’rith, 1957.

<sup>5</sup> Williams, *op. cit.*, p. 454 (italics in original).

<sup>6</sup> Gordon W. Allport, *The Nature of Prejudice*, Cambridge, Mass.: Addison-Wesley, 1954, p. 489.

structure, and educational activities are highly valued in the countries with which exchange programs have been established.

Our own research data from a study of American Fulbright grantees from nine midwestern states indicate that contacts devoted to common professional concerns spread beyond the immediate university setting while the grantees are abroad and continue after their return.<sup>7</sup> All but two of our sample of 217 Senior Scholars reported frequent face-to-face contacts with professional colleagues while abroad, and on the average each grantee had maintained such contacts with over 20 foreign colleagues. In addition, almost 60 per cent of the grantees had collaborated with foreign colleagues or students on research.

The enduring nature of these relationships is shown by the fact that 92 per cent of the Research Scholars and 82 per cent of the Lecturers had maintained their professional contacts abroad, although the average length of time since their return home had been four years. Over half of the grantees had made donations to the libraries of their host institutions, almost half had arranged lectureships or fellowships for their colleagues or students abroad to come to American universities, and 75 per cent had tried to do so. Over half of these scholars had arranged correspondence between their American colleagues and students and professionals whom they had met abroad. In some cases, fairly large networks of persons conducting research on common problems had been established. For example, one agricultural chemist remarked, "I've put people in our stations in touch with about 20 persons carrying on important research in New Zealand, Australia, and England." Eighty-five per cent had referred Americans going abroad to friends and former colleagues at their host universities.<sup>8</sup> Since their return, approximately 80 per cent of the grantees had en-

tertained in their homes colleagues from their host institutions and others whom they had met overseas or who had been referred to them by friends abroad.

Allport has observed that acquaintance programs are most likely to be effective and positive when they lead to "a sense of equality in social status."<sup>9</sup> Williams qualifies this assertion:

If we take "equal status contacts" to be those in which it is reciprocally understood by the participants that each has a right to be accorded roughly equivalent consideration, respect, deference, and right to be heard, it would seem that such contacts are especially likely to produce friction if (a) the basis of equality is insecure or uncertain, or (b) the goal-structure of the interaction favors competition.

What does seem likely even under the latter conditions is that the participants tend to become *important* to one another. It is difficult for equal-status contacts to occur repeatedly without some affective involvement. . . . Thus the relationships are likely to become both affectively charged and affectively and conceptually complex. . . . If this hypothesis holds, equal-status contacts may be assumed to change stereotypes, at least in so far as the immediate objects of interaction are concerned.<sup>10</sup>

One quality of the status of Visiting Professor occupied by the Fulbright grantee, as noted above, is its relative equality with that of his peer group in the host institution—in the very conditions listed by Williams. Since the Visiting Professor is not a permanent member of the host university and hence does not become a competitor of serious concern to other professors within that system, the situation may easily be structured so as to encourage cooperation with him or indifference to his presence. Since he is not a threat, his occupancy of the status of Visiting Professor is usually sufficient validation of his right to it. His legitimized rank makes his position salient to his colleagues; thus cooperation is more likely than indifference. It would seem, then, that the Visiting Professor occupies a very favorable position to initiate positive modifications of attitudes of those in the host institution.

According to our respondents, there is at least one respect in which the Visiting Professor is more favored than his hosts. The permanent Professors in a number of countries cannot easily modify their relationships with their students so as to permit more informal interaction and thus, perhaps, more effective teaching and learning. Such behavior as inviting students to their homes would be considered unduly deviant by their colleagues in many universities abroad. But greater flexibility exists for the Visiting Pro-

<sup>7</sup> The complete study is reported in John T. Gullahorn and Jeanne E. Gullahorn, *Professional and Social Consequences of Fulbright and Smith-Mundt Awards*, Washington, D. C.: International Educational Exchange Service, U. S. Department of State, 1958 (Mimeographed). We are grateful to the Program Evaluation Staff, International Educational Exchange Service, Department of State, for sponsorship of the research; and to the University Research Fund, University of Kansas, and the Michigan State University All-University Research Fund for financial support of additional analysis of data.

<sup>8</sup> Since our data are based primarily on reports from American Fulbright grantees, an adequate representation of the assessments and feelings of the host nationals is not possible. This area warrants further investigation.

<sup>9</sup> Allport, *op. cit.*, p. 489.

<sup>10</sup> Williams, *op. cit.*, p. 441 (italics in original).

fessor: his entertainment of students at home tends to be viewed as *variant* role behavior; while it is not approved, it need not necessarily upset his relationships with faculty colleagues. To use Parsons' terms, in changing from the affectively neutral, universalistic, functionally specific lecturer-student relationship to the affective, particularistic, more diffuse relationship of host-guest, the Visiting Professor can interact with students within a context where modification of attitudes is more likely. Data from our study indicate that the Visiting Fulbright Professors did establish these more permissive relationships with individuals abroad. The average person in this group reported close friendships with over fifteen foreign citizens; furthermore he was entertained in the homes of from fifteen to twenty foreign citizens. In addition, he met outside the classroom with over fifteen foreign students.<sup>11</sup>

Our data also point up the significance of the fact that academic men in America and in most of the rest of the world share certain common values and norms. A slight modification of hypotheses advanced by Homans would lead us to

<sup>11</sup> The large number of "close friendships" reported no doubt reflects to some extent the tendencies toward exaggeration as well as the gregariousness of Americans and the relative ease with which they feel they establish relationships with others. Ninety-one per cent reported, however, that they had maintained these friendships through correspondence since their return to America. Comparable data from the grantees' overseas counterparts would be particularly valuable, especially since, as Lewin noted in *Resolving Social Conflicts*, New York: Harper, 1948, Chapter 1, Americans' perceptions of a "close friendship" relationship differ from those of individuals in other cultures.

expect that in a situation of shared values and norms increased interaction leads to strengthened positive sentiments and *vice versa*.<sup>12</sup> Information provided by a "Satisfaction" Scale based on questionnaire data from the study indicates a significant positive relationship between the number of foreign professional and student contacts reported by grantees and their own degree of satisfaction with the award experience.<sup>13</sup> Further, the stronger the positive sentiments indicated on the Satisfaction Scale, the more likely was the grantee to continue interactions with former colleagues and students from abroad and to initiate correspondence and visits between his American and foreign colleagues.

In conclusion, it should be noted that the position of Visiting Professor in the structure of the university abroad simplifies, at least in some measure, the initiation of formal contact with students and colleagues. But if the status incumbent's interactions are to lead to changed perceptions of Americans, he must go further. As Williams states, "The greatest likelihood of 'positive' attitudinal changes attendant upon intergroup interaction is to be anticipated when the relations of the participants are informal, cooperative, noncontrived, and recurrent over a relatively long period."<sup>14</sup> It is exactly this kind of relationship that the privileged position of "equal-status stranger" makes possible for the Visiting Professor.

<sup>12</sup> George C. Homans, *The Human Group*, New York: Harcourt, Brace, 1950, Chapters 5 and 6.

<sup>13</sup> The Satisfaction Scale was constructed on the basis of 13 questionnaire items concerning grantees' evaluations of the consequences of their award experiences.

<sup>14</sup> Williams, *op. cit.*, p. 444.

## NEWS AND ANNOUNCEMENTS

### JAMES H. S. BOSSARD 1888-1960

James Herbert Siward Bossard was born September 29, 1888, at Danielsville, Pennsylvania, and died January 29, 1960, in Philadelphia. He received his Bachelor's degree from Muhlenberg College in 1909, the M.A. in English from the University of Pennsylvania in 1911, and the Ph.D. in Sociology, also from the University of Pennsylvania, in 1917. He taught at Muhlenberg College from 1911 to 1917, and at Lafayette College from 1917 to 1920. He then came to

the University of Pennsylvania, where he served as a member of the Faculty for 40 years, and became Professor Emeritus in 1959. As visiting professor, Dr. Bossard also taught at the University of California and Yale University. Muhlenberg college awarded him the honorary degree of L.H.D. in 1949. From 1938 until his death he was Director of the William T. Carter Foundation for Child Development at the University of Pennsylvania. He was in addition appointed Professor of Sociology in Psychiatry at the School of Medicine of the University of

Pennsylvania in 1950. He served as President of the Eastern Sociological Society in 1934, and was also an officer of the American Sociological Society. In a professional career of fifty years he was active in many organizations at local, state, and national levels.

Professor Bossard was for 50 years continuously engaged in teaching, research, and writing. His 30 books include *Problems of Social Well-Being* (1927), *Social Change and Social Problems* (1934, 1938), *University Education for Business*, with J. Frederic Dewhurst (1931), *Man and His World*, with others (1932), *The Sociology of Child Development* (1948, 1954, 1960), *Ritual in Family Living*, with Eleanor S. Boll (1950), and *The Large Family System*, with Eleanor S. Boll (1956). In his later years his audience was widely extended, and he contributed extensively to national magazines. He was engaged at the time of his death in a two-year study of juvenile delinquency for the Greater Philadelphia Movement. He was, in fact, then at the beginning of his nominal retirement, probably at the peak of his professional activity and productivity, and was more than ever in demand for writing, lecturing, research, and for administrative work.

As senior member of the Sociology Department, Professor Bossard was for many years the link between his juniors and such founders of the Wharton School as Simon N. Patten, Carl Kelsey, and James P. Lichtenberger. He was widely respected as a scholar and valued as an advisor for students and colleagues. By the testimony of many graduate students he was without peer as a master of the narrative method in classroom and lecture hall. Both his private life and his professional activity were marked by a patience, a continuity, and a sense of history that came from his confidence in the relevance and significance of his work. As a man of powerful personal imperative, he was profoundly convinced of the reliability of his materials, the efficacy of his methods, and the validity of his goals. To those colleagues who shared his activities over the years he appeared a singularly judicious and complete man.

VINCENT HEATH WHITNEY  
University of Pennsylvania

## LOUIS H. BLUMENTHAL 1893-1959

Louis H. Blumenthal, for 35 years the Executive Director of the San Francisco Jewish Community Center, died on April 24, 1959. He was born in New York City and received his formal education at the College of the City of New York and Washington Square College, New York University. The breadth of his activities, teaching, and writing reveal a rare combination of social philosopher, administrator, and community leader. Louis Blumenthal was one of the pioneers in seeking to discern the contributions of the social sciences in the twenties, thirties, and forties and to embody these insights in administration, group work, and community organization.

His published books include *Group Work in Camping*, *Administration of Group Work*, and *How to Work With Boards and Committees*. He wrote numerous chapters for such volumes as *Practice of Group Work*, *A Decade of Group Work*, *Group Work and the Social Scene Today*.

Mr. Blumenthal conducted courses and seminars at the University of California Extension Division, Stanford University, University of British Columbia, and San Francisco State College. His service on national, state, and local advisory committees was extensive. Louis Blumenthal was one of a small company of advanced guard who sensed the creative currents in the developing behavioral sciences and worked for their application in institutional and community change.

ROY SORENSON  
San Francisco, California

## JACOB KATZ 1922-1959

Jacob Katz, Lecturer in Sociology at Boston University College of Liberal Arts, died suddenly on November 4, 1959, at the age of thirty-seven. Survivors include his widow, Mrs. Freda Tambor Katz, and infant daughter, Julie.

Mr. Katz, born and educated in Boston, was a veteran of World War II. He received his M.A. in Sociology from Boston University in 1948 and subsequently became a



candidate for the doctorate at Columbia University. An eager student of sociology, his simultaneous interest in public health had far-reaching influence on his career. At Columbia University, Mr. Katz became professionally affiliated with the School of Administrative Medicine and Public Health, both in the Division of Epidemiology and in the Institute of Administrative Medicine. His work with Drs. Leonard S. Rosenfeld and Avedis Donabedian on Medical Care Evaluation Studies under the Health, Hospitals, and Medical Care Division of United Community Services of Metropolitan Boston was well received. He took time out from a busy research career to earn a Master of Science degree from the Harvard School of Public Health in 1958, after which he became Director of the Survey of Nursing Needs and Resources of Massachusetts. His other awards include a Russell Sage fellowship and Columbia University and Social Science Research Council grants for research in health. He published articles in several national medical journals.

This public record might suggest that Mr. Katz's sociological concerns were subsidiary to his interests in the health field as such. Actually, the application of sociological principles and research techniques to public health problems was his special interest. He was throughout his career a social scientist.

Jacob Katz had an enviable facility for appreciating life and all it offered—friends, books, or a pot of coffee! He was as friendly individually as he was meticulous professionally. His family, friends, colleagues, and students will miss his warm personality.

THERESE M. MALCOLM

*Boston University*

**Congres International de Sociologie.** The 19th Congress, the first on the American continent, will be held in Mexico City August 3–September 6. Lic. Carlos A. Echanove T., San Juan de Letran 9–207, Mexico, D.F., is President of the organizing committee; Carle C. Zimmerman, 200 Emerson Hall, Harvard University, Cambridge 38, Massachusetts, is Secretary for the United States and Canada. The subject of the Congress for this year will be "Themes de Sociologie Latino-Americaine." Interested persons should correspond with Professor Zimmerman.

**European Society for Rural Sociology.** The Second European Congress of Rural Sociology will

be held August 1–6, 1960, at the Agricultural College of Norway. Congress papers and discussions will focus on the changing structure and functions of rural communities, rural organizations in a changing society, and changes in rural occupational structure and labor organization. The Congress is being organized by the Norwegian Society for Rural Sociology. Information may be obtained from the Congress Secretariate, Postbox 53, Vollebakk, Norway.

**UNESCO.** The North American Conference on the Social Implications of Industrialization and Technological Change will convene at Chicago, September 6–13, under the joint sponsorship of UNESCO, the United States and Canadian National Commissions, and the University of Chicago. Bert Hoselitz, University of Chicago, is the North American organizer of the meeting, H. M. Phillips is the UNESCO organizer, and Wilbert Moore, Princeton University, who has recently been appointed to the Commission, will serve as general rapporteur.

Samuel P. Hayes, Jr., Professor of Economics at the University of Michigan and Director of the Foundation for Research on Human Behavior, has been appointed Chief of the Social Science Department of the UNESCO Secretariat in Paris for a two-year term beginning early this summer.

**American Association for the Advancement of Science, Section K.** Section K—Social and Economic Sciences—will hold sessions for contributed papers at the annual meeting in New York, December 26–31, 1960. Association members interested in presenting papers at these sessions should forward titles and abstracts not later than September 1 to Donald P. Ray, Secretary of A.A.A.S. Section K, National Institute of Social and Behavioral Science, George Washington University, Washington 6, D. C. Papers should be based on research recently completed by the author. The American Sociological Association is an affiliate member of the Section on Social and Economic Sciences of the A.A.A.S.

**American Council of Learned Societies.** Among recipients of grants awarded for research in the humanities and related social sciences are: Charles H. Tilly, Department of Sociology, University of Delaware: Study of the social origins of the Vendée; and Everett K. Wilson, Department of Sociology, Antioch College: Development of French social thought as seen in *L'année Sociologique*: 1896–1956.

**Bureau of Social Science Research, Inc.** At a recent meeting of the Bureau's members, the following Trustees were reelected to serve through 1960: W. Phillips Davison, Robert T. Bower, Ellsworth Bunker, George Gallup, Paul F. Lazarsfeld, Frank Lorimer, and Herbert J. Miller, Jr. At a subsequent meetings of these Trustees, Davison was elected Chairman; Bower, Vice-Chairman and Director of the Corporation; Ivor Wayne, Secretary and Assistant Director of the Corporation.

**National Safety Council.** The Metropolitan Award of 1,000 dollars for research in accident prevention has been announced. Research may be sub-

mitted by the investigator, may be invited by the Council Committee on Research and Education, or may be nominated by anyone familiar enough with a research project to do so. For further information, write to John J. Flaherty, Director, Research Division, National Safety Council, 425 North Michigan Avenue, Chicago 11, Illinois.

**Wainwright House.** Graduate scholarships are being offered in conjunction with a seminar on "Science and the Total Nature of Man" to be held on October 28-30 at the Wainwright House, Rye, New York. Graduate students are invited to submit essays on "How Can New Frontiers of Science Further the Study of the Nature of Man?" to Mr. Weyman Huckabee, Secretary, Wainwright House, Rye, New York, by July 1, 1960.

**American University of Beirut, Lebanon.** George C. Fetter was appointed Chairman of the Department of Sociology and Anthropology at the beginning of the academic year. He recently returned from a study tour of community development and rural improvement programs in Kenya, Uganda, the Sudan, and Ethiopia, and is presently conducting a "Study of Attitudes Toward Technological Change and Rural Improvement in the Bekaa Valley" (Lebanon) under a grant from the Rockefeller Foundation.

George H. Weightman has joined the staff as Assistant Professor and is working on a study of "Social Attitudes as a Function of Social Status in Three Selected North Lebanese Towns."

Gene B. Petersen joined the faculty in February after spending two years in Egypt on a Ford Foundation grant, and Samir G. Khalaf was appointed to the staff on a part-time basis.

**The University of Arizona.** Ralph R. Ireland has resigned his post as Head of the Department; Raymond A. Mulligan has been appointed Acting Head. The staff of the Department includes: Frederick A. Conrad, Donald S. Klaiss, Raymond A. Mulligan, and Robert C. Stone, Professors; William C. Lawton, and Sanford W. Shoults, Associate Professors; I. Roger Yoshino, Assistant Professor; Jessie Carnevale, Julia Fuller, Bernard Garmire, Joseph R. Hambenne, Pauline Mahar, James E. Officer, and Robert B. Rowen, Instructors.

**Barnard College.** Mme. Maria Ossowska, Professor of Sociology at the University of Warsaw, served as Virginia C. Gilmerlee Visiting Professor during the spring term. Following her stay at Barnard, she is spending three months visiting and lecturing at several other universities and colleges in this country, sponsored by the Ford Foundation.

**Bowling Green State University.** Evelyn Duvall and Sylvanus Duvall will conduct a Family Workshop from June 20 to July 2, 1960. Registration is open to qualified students, teachers, and counselors. Inquiries should be sent to Dr. Donald S. Longworth, Chairman, Sociology Department, Bowling Green State University, Bowling Green, Ohio.

**University of California, Los Angeles.** Donald R. Cressey, Chairman of the Department and President of the Pacific Sociological Association, is completing a monograph on social organization of correctional agencies for the Russell Sage Foundation.

Wendell Bell has received a grant from the Carnegie Corporation of New York for a program of training and research on leadership in a new nation, the Federation of the West Indies. During the more than three years that the program will run, advanced graduate students from UCLA will engage in field work in the West Indies and West Indian students will come to UCLA to pursue graduate work in sociology and anthropology. Also, Bell has been elected Vice President (Southern Division) of the Pacific Sociological Association, and he is completing, with Richard J. Hill and Charles R. Wright, a report on public leadership in the United States for the Fund for Adult Education.

Richard T. Morris, on sabbatical leave for the 1960 spring semester, is working on American value systems and occupational situs correlates.

Appointed to the sociology staff in 1959-1960 were John E. Horton and Virgil Williams, Jr., Acting Instructors, and Melvin Seeman, Associate Professor. Seeman (in conjunction with J. B. Rotter and S. Leverant of Ohio State University) has been awarded a grant by the Air Force Office of Scientific Research to pursue research on the problem of alienation.

Richard J. Hill has accepted appointment with the Department of Sociology at the University of Texas beginning July 1, 1960, and Ernest M. Willis is taking a research position with the Cleveland Psychiatric Institute at Western Reserve University.

**Chico State College.** Ritchie Lowry, Assistant Professor of Sociology, is conducting a study of "The Power Structure of a Northern California Community." Ronald Kurtz, Instructor of Anthropology, is completing his study of culture among the Conyoncito Navajo.

**Florida State University.** Ivan Nye, Director of the Sociological Research Laboratory at Washington State University, will join the staff as Professor of Sociology and Director of the Interdivisional Doctoral Program in Marriage and Family Living, beginning September 1, 1960.

**Franklin and Marshall College.** Robert F. Eshleman was appointed Chairman of the Department of Sociology and Anthropology last September, following the death of the Acting Chairman, Ivan W. LeFevre. Jetse Sprey was appointed Instructor, and Charles H. Holzinger was promoted to Associate Professor.

**University of Georgia.** Raymond V. Bowers has been appointed Chairman of the Division of Social Sciences in addition to his position as Head of the Department. He also has a research contract from the Air Force Office of Scientific Research on the impact of technological change on executive and professional careers in large scale organizations. Mabel A. Elliott, on sabbatical leave from Chatham

College, was Visiting Professor during the winter and spring quarters, and Leslie J. Silverman joined the staff last September as Assistant Professor. Rollin Chambliss has returned after a six-months residence at the University of Dacca as a Fulbright Scholar.

**Long Beach State College.** The Sociology Department now consists of eight full-time and three part-time staff members, including Nick Massaro (Chairman), David Dressler, George Korber, William Hartman, Paul Ullman, Albert Sheets, John Dachawich, and Barbara Day.

**Los Angeles State College.** Robert Ewald and Louis Faron have received a grant from the National Science Foundation to undertake a preliminary ethnographic survey of the Choco and Cuna Indians of Eastern Panama during the summer of 1960.

Don J. Hager has been appointed by the State Attorney-General to the Advisory Committee, Constitutional Rights Section, Department of Justice, State of California. The Advisory Committee has responsibility for initiating research, surveys, and other inquiry to promote and maintain civil rights in housing, employment, education, and public accommodations.

**University of Massachusetts.** Arnold Levine has been granted a leave of absence. Martin Vogel-fanger has been appointed to the rank of Instructor.

**McMaster University** (Hamilton, Ontario). The Department of Sociology held a conference last February on kinship, marriage, and the family. Papers were read by David M. Schneider, William A. Westley, H. David Kirk, and Peter C. Pineo.

**Oberlin College.** George E. Simpson, Professor of Sociology and Anthropology, has received a research grant from the National Institute of Mental Health for a study of religious cults in Trinidad, West Indies. He will be on sabbatical leave during the first semester of 1960-1961 and expects to be in Trinidad from June 15 to December 15.

**University of Pennsylvania.** Edward P. Hutchinson is Chairman of the American Immigration Conference Research Committee which is initiating a semi-annual digest of studies of immigration and the foreign-born.

Richard D. Lambert is Director of the study, Attitudes of Western and Asian Peoples Toward the Accumulation of Wealth and Savings, sponsored by UNESCO. He helped to organize and attended a series of meetings on the topic in Calcutta, India, in December.

Leonard Savitz is directing a research project on Negro criminality for the Philadelphia Commission on Human Relations.

**University of Wisconsin.** Howard Becker, together with Frances Bennett Becker, spent the past summer in Europe, continuing research on Greek and on Hessian mentality and social structure as well as lecturing at German universities.

Marshall Clinard is on leave for a second year in India as consultant to the Ford Foundation's Urban Community Project.

Burton R. Fisher has been appointed Chairman succeeding William H. Sewell, who is spending this year as a Senior Fellow at the Center for Advanced Study in the Behavioral Sciences. Fisher is serving as Chairman of the Executive Committee of the Wisconsin Survey Research Laboratory. Albert J. Reiss, Jr., now at the State University of Iowa, will join the Laboratory in July as Director and the Department as Professor of Sociology.

The Marx-Engels-Lenin Institute in Moscow is preparing an edition of the same Minutes that Hans Gerth recently translated and published, *The First International: The Minutes of the Hague Conference of 1872*.

Horace Hamilton of North Carolina State College is a Visiting Professor teaching courses in Rural Sociology. Hamilton is President-elect of the Population Association of America.

Douglas Marshall is Chairman of the NC-18 Regional Committee on Population Research.

Arthur Wileden has been elected Chairman of the North Central Rural Sociology Committee, sponsored by the Farm Foundation and the Land Grant College Association.

Eugene Wilkening was on leave in 1959 at the University of Melbourne, Australia, as a Fulbright Research Scholar participating in research on acceptance of innovations among Australian farmers.

Frank Hartung of Wayne State University has been visiting for the current school year, teaching courses in criminology and social disorganization.

Robert McGinnis and Lyle W. Shannon are continuing their research project on Value Assimilation Among Immigrant Workers, with funds supplied by the National Institute of Mental Health and the Urban Research Committee of the University of Wisconsin. Additional support has made it possible to broaden the study so as to include samples of immigrant Southern Negroes and Southern Whites as well as Mexican-Americans.

Leo Schnore, formerly of the University of California, Berkeley, has been appointed Associate Professor. Currently he is serving on the Committee on the 1960 Census of the Population Association of America and on the Committee on Urbanization of the Social Science Research Council.

Robert C. Davis is a member of the Committee on Space Law and Sociology of the American Rocket Society. He has also been elected to the executive committee of the Wisconsin Sociological Society.

Eugene Friedmann is Research Consultant to the Planning Committee of the White House Conference on Aging to be held in 1960.

Louis H. Orzack is Chairman of the Committee on Occupations of the Society for the Study of Social Problems. He is currently engaged in a study of role emergence and role learning during organizational growth in an institution for mentally retarded children.

## BOOK REVIEWS

*The Freudian Ethic.* By RICHARD LAPIERE. New York: Duell, Sloan & Pearce, 1959. x, 299 pp. \$5.00.

*The Concepts of Sigmund Freud.* By BARTLETT H. STOODLEY. Glencoe, Ill.: Free Press, 1959. xii, 274 pp. \$6.00.

These books are both concerned with Freud's thought. They differ almost entirely in substance and in spirit. Yet ultimately they are both parts of larger and still unwritten works.

Stoodley's book—well designed by the Free Press—is a serious and sustained explication of original texts. It is indeed a study of Freud's concepts. The explication is driven on by the conviction that Freud poured much new wine into old bottles. Freud's *scheme*, with its enduring attachments to mechanical formulations, is actually burst by Freud's *ideas* and *images*: their logic often remains latent, like a dream's. When it is made manifest, Freud stands revealed as having become more of a sociologist than he was ever apparently prepared to see himself. This, in crude essence, is Stoodley's immediate theme. He argues and illustrates it responsibly and well. With a classic liking for trinities. Stoodley shows that there were three major stages through which Freud moved in order to leave an earlier somatic reductionism for an ultimate acknowledgement that the mind not only has "a mind of its own" but that the ego also has its ideals. In part these come from a social universe.

This movement, of course, was a struggle, the more so since Freud remained attached to the past while allowing "periodic insights" to "out-strip plodding theory." Theory moreover was asked to "routinize" the insights of its progenitor. In part the ambiguities of the struggle were to be resolved by the notion of mental mechanism. And mechanism, as we know, became distinguished into several forms. Repetition—compulsion, regression and repression, engage Stoodley in particular. Like the other mechanisms they were to describe the constituent elements of the flow and of the (potential) arrest of the inner life; together with other concepts they were also to yield an intellectual sense of the coherence of these elements.

Before long their status as ideas is half forgotten and through Freud they confront us as facts. Still, the pattern of concepts grows. As a pattern it can no longer be dissolved or dismissed. Increasingly Freud acknowledged the

coherent ego, even if he remained in the insistent presence of past attachments to somatic and material modes of thought. Cognitively at least, he becomes the architect of a house of many styles. As it grows, the house has ever more room for a free or freed individual. Freud's own ambiguities, Stoodley suggests, allow him to leave us with an image of ourselves as living and working on middle ground. We become conceived as creatures of more than one kind of order: the mind includes a social self which in turn has a natural history.

The fearless effort at transforming clinical impression, itself guided by intellectual anticipations, into a reasonably coherent web of quite delimited ideas does in the end allow—and at a time when Freud himself, incidentally, is more than ever a refugee—for human beings as active masters of their life: the wish to discover can be accommodated in the terms chosen for the formulations of discovery.

In the course of arguing this general thesis, Stoodley raises many specific issues which deserve detailed discussion elsewhere. He suggests, for instance, that the "introduction of the ego-ideal gave the coup de grace to libido theory" (p. 240) or that, for all its richness and depth, Freud's thinking is often a "mixture of scholasticism and opportunism . . . combined with the creation of an ingenious mythology" (p. 241). But Stoodley is never a detractor: he wants to create intellectual order. He wants to help define what needs be ordered, given a commitment to a sociological perspective. His masters are the "triumvirate—culture, social organization and personality" (p. vii). I would have preferred him not to leave the body out. I could also have wished for more concern with the clinical and empirical "facts" (such as the *imagined* experiences of hysterics with their past social world) as the counterparts to Freud's concepts.

In the end this very worthwhile book may have one successful enemy: time. Stoodley's ideas were first written down about ten years ago, in the form of a dissertation deposited in the archives of Widener. Since then very much has been said in honor and extension of Freud along lines quite parallel to those reasoned by Stoodley. Erikson and Rieff are probably the best examples. Jones' three volumes, though close to the "orthodox" tradition, are also a biography of Freud's thought. Less rightly than wrongly, Stoodley's book will have to live in their shadow, even if his creative and critical



exegetical results stand rather alone in many respects.

Mr. LaPiere's book is a moral essay. In an urbane and engaging style he insists on a fairly bizarre diagnosis. He is concerned with the popularity of Freudianism today. Freudianism includes the social implications of the thought of Freud and his followers. These implications, in turn, include an ethic, the Freudian ethic:

As a code of conduct the Freudian ethic is entirely negative. It is composed of sentiments and attitudes regarding man's capabilities that, if literally applied would keep him from attempting to devise anything new. For it makes the world about him a hostile and inhospitable place . . . this world has been "internalized" to the end that a part of himself intimidates the rest. There is therefore no escaping psychic agony (p. 63).

This ethic is associated with a process of healing and an emphasis on the subjective. In the last regard it has much in common, according to LaPiere, with Christian Science. Yet unlike the latter it refuses to allocate any responsibility to the patient. Indeed the Freudian ethic is the antithesis of the Protestant ethic. It is an ethic empty of the values of self confidence, personal integrity, self reliance, or moral courage. But then the milieu of Freud, Vienna, was in "sociological terms disorganized and demoralized. . . . In lay terms it was a city of sin."

We are witnesses and protagonists of a struggle between the Protestant and the Freudian ethic. The propagation of the Freudian ethic proceeds in association with other developments, including the emergence of the organization man. These it did not cause; rather it is in "concordance" with them. As a result, however, we are now faced with permissive homes, progressive schools, "contra-social" children, the ideal of adjustment, coddling on the campus, and a failure to see that "if the criminal does not pay for his crime, the rest of us certainly will" (p. 170). Besides, the Freudian ethic has helped in the decline of an old and the rise of a new bourgeoisie, which typically marries early, operates "not upon the basis of principles, but upon hope and faith," may end as dependents on social charity, makes no sacrifices for its ideals, and is generally without prudence. Indeed, in LaPiere's view, there are good and bad men. The disciples of Freud do not belong to the first group. The good are men of enterprise and responsibility. When they become shorn of power, society begins to abandon the principle of reciprocity. We may then see a constant rise in the proportion of the irresponsible. As a result the rate of social change will diminish and we shall languish. Only if we change and widely reconfirm older attachments to self reliance can a dismal future be avoided.

In the main, of course, Mr. LaPiere does not

wish to diminish the vigor of his warning by confronting it with evidence. In the absence of any interplay between facts and claims, one is then confined to the observation that his avowed view of the complexity of truth is quite lost behind the simple moral alternatives which he urges on us. To label one of the alternatives "Freudian" further confounds the matter. Convenience is surely no excuse to a man serious about Protestant responsibility.

Like Mr. Stoodley, Mr. LaPiere has not finished. Stoodley wants eventually to understand the congruence of Durkheim and Freud, considered as the result of two individuals' intellectual change; LaPiere is interested in the intricacies—"ideological, procedural, organizational"—of social change in the large. May their individual enterprises be responsible enough not to by-pass each other.

KASPAR D. NAEGELE

*University of British Columbia*

*Ethics and the Social Sciences.* Edited by LEO R. WARD, C.S.C. Notre Dame, Ind.: University of Notre Dame Press, 1959. xiii, 127 pp. \$3.25.

Here are six essays, fruits of a 1957 conference at Notre Dame on ethics and the social sciences. The authors are Francis G. Wilson, Kenneth E. Boulding, Christopher Dawson, David Bidney, Herbert Johnston, and James R. Brown. They are two political scientists, one economist, one philosopher, one anthropologist, and one historian. There are no psychologists and no sociologists (unless one counts Christopher Dawson, the distinguished Roman Catholic historian as a sociologist, too).

The essays do not always meet the same issues. Mr. Boulding, especially, in a fascinating piece called "The Knowledge of Value and the Value of Knowledge," goes his own way. But the amount of agreement is substantial, and includes Mr. Boulding on the main problem.

There is no attempt to inject values into the social sciences; the authors are clear that the study of values by social science is empirical. Nor is there an attempt to base ethics solely on the conclusions of social science. Ethics, however, is regarded as a rational endeavor; the authors argue either that reason can discover natural law (this is the stand of the Thomists), or that matters of value are arrived at in very much the same way as matters of fact (argued especially by Messrs. Bidney and Boulding). For all the authors, values can be validated and defended rationally.

Agreement seems to center on three propositions. (1) There are universal (and, for some, absolute) values. (2) These values are discov-

erable by human reason. (3) A major social value is the existence of a single world-culture. The third proposition follows easily enough from the first two, requiring only one additional step, namely, the assumption that the culture of a people and its values have a kind of unity. Indeed, one way to summarize a culture is to state its values. So the implicit argument is approximately this: since universal values can be discovered and validated, there are correct universal values (including, of course, a single, correct ethic), and incorrect ones. The culture that best embodies the correct values is the proper, or correct, culture for all men. Hence we are obligated to bring it about.

There is a good deal of talk in this book about an "inter-faith" (that is, Catholic and Protestant) approach to ethics. But despite this, we are compelled to ask whether the single, proper culture for all men would be thought of as having a single, correct religion? Of course, one might argue that all religions teaching the correct ethic and holding the correct values would be permissible. But there would still be a question whether more than one such religion is ever possible. If the authors were to hold that there could be more than one, it is hard to see why they think there should be a single world-culture. For more than one culture might, by the same argument, embody the correct values. And we would be back at a kind of cultural pluralism, which the authors seem to oppose in any form.

The enemy, as some of the authors see it, is "positivistic" or "empirical" social science. Still, it is not the findings of such science that are condemned, but a presumed denial that values can be discovered, validated, or argued for. Surely there are social scientists who believe that values are "subjective" and are reducible to taste, preference, or the results of conditioning. Yet that is not a problem in social science at all, but in philosophy. So the real "enemy" turns out to be one kind of philosophical empiricist working in the social sciences, who is usually politically liberal as well. (Approved citations on politics in text and footnotes are from the most conservative political scientists.) Unfortunately, much of the polemic, and some of the positive argument, is sheerly rhetorical or question-begging.

RALPH ROSS

University of Minnesota

*The Idea of a Social Science and Its Relation to Philosophy.* By PETER WINCH. London: Routledge & Kegan Paul; New York: Humanities Press, 1959. 143 pp. \$2.50.

This little book can be read for both amusement and profit by the sociologist who is com-

mitted to his discipline yet occasionally wonders how much progress it has actually made. There are sound insights in the book, but they are often expressed and thought out in ways that will impress the sociologist as naive and groping.

Symbolic interactionists, in particular, will find much to agree with. Contending that the province of philosophy is concern with "the nature of reality as such and in general" (p. 8), Winch says, "Our idea of what belongs to the realm of reality is given for us in the language that we use. The concepts we have settle for us the form of the experience we have of the world" (p. 15).

His stated purpose in writing the book is "to show that the notion of a human society involves a scheme of concepts which is logically incompatible with the kinds of explanation offered in the natural sciences" (p. 72). Two principal targets for his criticism are Theodore Newcomb and Vilfredo Pareto. Newcomb's social psychology text is condemned for an overly physiological conception of *motives* in human action. Pareto's analysis of *residues* underlying non-logical actions provides a whipping boy for Winch in his attempt to show that the social scientist cannot discriminate between classes of homogeneous events according to the same rules as the natural scientist. Such discrimination (or classification) is, he argues, the core of science, but "what the sociologist is studying, as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist's investigation, which specify what is to count as 'doing the same kind of thing' in relation to that kind of activity" (p. 87). As an example, he cites Pareto's analysis of baptismal rites, noting that a Christian would strenuously deny their equivalence in character to the letting of sacrificial blood or the sprinkling of lustral water by a pagan. He seems to assume that the sociologist would not think of *using* such a denial as an additional datum in his study of religious behavior.

This, and other portions of Winch's argument amount to little more than an elaborately obfuscated version of the old notion about the social scientist being *within* (and therefore hamstrung by) the subject matter he wishes to study. The sophisticated sociologist will have no difficulty in formulating his own rejoinder to this and the numerous other errors and inconsistencies in the book. He may find the effort justifiable as an exercise in professional self-consciousness. In the course of this exercise he will be amused to realize Winch is essentially struggling to prove that there is, after all, still a place for philosophy despite the encroachments of scientific sociology.

We have, it seems, come a long way since the days when sociologists had to devote a substantial proportion of their writing to defining a special subject matter that could belong exclusively to sociology!

WILLIAM R. CATTON, JR.

University of Washington

*Man's Way: A Preface to the Understanding of Human Society.* By WALTER GOLDSCHMIDT. New York: Henry Holt & Co. (Holt-Dryden Book), 1959. 253 pp. \$2.90.

This short, disarmingly well written book by the retiring editor of *American Anthropologist* may prove, ten years hence, to have been something of a bombshell. The various schools of theory in anthropology have been moving toward one another, in spite of sniping and rearguard action, for some time. Goldschmidt has been the first to achieve an effective statement which includes all or most of the several theoretical positions. Where he has not done so, because of the nature of the book's organization, he has made the task infinitely easier for someone else to achieve. The author has not been merely eclectic; these theories are shown to be necessary to each other. He has also maintained the magnificence and relevance of the sweep of time, space, and variety that anthropology—like history, but alone among the social sciences—has learned to command. The most controversial aspects of evolutionary theory and the no longer controversial elements of functional theory are shown to be of a piece. If, after this book, evolutionists, functionalists, and *apologistes pour l'histoire* cannot communicate, it will be the sheerest willfulness on their part.

For one purpose or another, I have now read *Man's Way* three times. The first time I read it, I was made to realize that it is possible to derive evolutionary theory from functional axiom. The second time, I realized that Lewis Henry Morgan was a functionalist in all but name, and that Leslie White and the evolutionists, on the one hand, and Herskovits as well as others who have stressed historicity and cultural relativism, on the other hand, stem equally from Morgan by legitimate and direct lines. Morgan's mistake was to follow the Darwinian pattern too closely and to assume that history and evolution overlapped in "survivals" in the same way in both social and biological spheres. The difficulties were in the "survival" concept, not the evolutionary theory or even the stages.

The third time I read the book, I realized that here is only a beginning, and that years of testing and examining the tenets of social and cultural process still lie ahead. In the past few decades it has at last been possible to get back

to this main business, which Morgan saw but proceeded to overlay with a host of false analogies and irrelevancies inherent in his *Zeitgeist*.

This book, more simply than any I know, brings out the elements in the necessary relationships between technology and social forms. Conversely, it is perhaps not so clear as some others (for example, White's *Science of Culture*) in showing the irreversibility of some technological achievements. It has not achieved—probably no book could as yet—the arduous task of examining in what senses social revolutions are irreversible, if indeed they are. But to make such statements is only to set in the widest context the book's very considerable achievements.

*Man's Way* is so much an anthropologist's book that there is a danger that it will be ignored by sociologists. Nothing could be more unfortunate at a time when the *rapprochement* between the two disciplines is at an all-time high. Some sociologists will object to the whole method implied in the word "understanding" in the subtitle. Others will find it lacking in what they call scientific rigor. Still others—but these the anthropologists will write off—are interested not in society, but only in modern Western society. Be all this as it may, for an easily manageable *aperçu* of where American social anthropology is going, no book can be more highly recommended.

PAUL BOHANNAN

Northwestern University

*A Reader's Guide to the Social Sciences.* Edited by BERT F. HOSELITZ. Glencoe, Ill.: Free Press, 1959. 256 pp. \$2.95 (paper); \$6.00 (cloth).

Rarely are the intentions of an editor so fully realized as those of Professor Hoselitz in this superb guide to the literature of history, geography, political science, economics, sociology, anthropology, and psychology. The initial conception developed out of the needs of library education, but was broadened to provide, in the words of the editor, "a general introduction to the literature of the social sciences that would deal with the differences in the literary output in the major disciplines and the nature of available tools, in the form of books, journals, pamphlets, and reference works, that are consulted and used by social scientists in their research and teaching." The volume succeeds eminently in serving as a systematic and intelligent guide to librarians concerned with the source materials of the social sciences, to social science specialists seeking an overall view of the literary output of a sister discipline, and to the general reader interested in an informed tour

through the fascinating wonderland of social science literature.

Each chapter on the seven selected disciplines includes, with varying degrees of emphasis, references to the historical development of the special area, appraisals of the more important classics in the field, a systematic review of current output, and critical comments on present trends and directions. Efforts have been made to report significant methodological developments as well as substantive contributions. Although the presentations are uniformly good, some convey more of a sense of intellectual excitement than others. Heinz Eulau's frankly behavioral approach to political science, for example, is an exceptionally enjoyable intellectual treat. He is incisive, yet constructive; serious, yet witty; inspired, yet sound; deliberately selective, yet judicious. His chapter abounds in brilliant insights and paradoxes. Thus, at one point, with admirable frankness he writes that "Political scientists are riding off in many directions, evidently on the assumption that if you don't know where you are going, any road will take you there" (p. 91). He concludes, however, that the future for a science of politics is bright.

By contrast, the contribution on sociology by Peter Blau and Joan Moore is more pedestrian. It also lacks balance. More than half of the chapter is devoted to the development of the discipline. The second part consists of comments on contemporary sociological literature in such selected areas as social theory, interviewing surveys, social psychology, demography and human ecology, social differentiation, and formal and informal organization. Significantly omitted are references to work in such fields as social disorganization and deviant behavior, the family, rural sociology, and the like. The result is a distorted picture of sociological output.

Special commendations must go to Professor Hoselitz. In addition to his contributions as editor, he prepared the chapters on history and economics and provided the volume with an unusually perceptive and scholarly introductory essay on the social sciences in the past two hundred years. It is an informed and fascinating study in the history of ideas, full of sensitive insights. One example is Hoselitz's observation on the significant role played by Montesquieu's *Spirit of Laws* in the development of basic social science viewpoints and methods. Hoselitz clearly sets forth the solid accomplishments of the social sciences. In the light of his historical perspective, he looks forward with confidence to the hope that "Comte's dream of a generalized science of man and society may be achieved in practice" (p. 25).

There are some forgotten men and works.

Galbraith in economics, for example, does not appear among the approximately 950 names in the index. And doesn't anyone use the *Encyclopedia of the Social Sciences* any more?

HARRY ALPERT

University of Oregon

*The Alienation of Modern Man: An Interpretation Based on Marx and Tönnies.* By FRITZ PAPPENHEIM. New York: Monthly Review Press, 1959. 189 pp. \$4.00.

In his "Reflections on the Fifties," Joseph Wood Krutch comments: "If 'progress' was still the key word and the key concept for the majority of Americans, 'alienation' was widely accepted as the key word and the key concept for an increasing number of intellectuals and artists" (*Saturday Review*, January 2, 1960). The remark is perhaps more apt than Krutch intended, for the idea of alienation is popular not only as a means for describing the intellectual's situation, but also as vehicle for the intellectual's indictment of modern society.

Dr. Pappenheim's book is another such indictment, in the tradition that began seriously with Marx and that, most recently, includes Fromm's *The Sane Society* and Kahler's *The Tower and the Abyss*. His thesis is that "the forces of alienation predominate in our era" (his italics, p. 16), and he analyzes this development of alienation in relation to the contemporary social structure. What emerges is a well written but quite standard account of man's troubled and inadequate self-definition in a commodity-centered, depersonalized society. The catalogue of spiritual troubles is familiar and complete: self-estrangement; anonymity; means-centeredness; indifferences; isolation; powerlessness; loneliness; calculation (as against spontaneity); meaninglessness; disenchantment.

Such troubles, of course, are none the less serious for being so familiar. Most of us would presumably agree with the author's value premise that "a society dominated by the forces of alienation stifles the fulfillment of human potentialities" (p. 17). The task of the analyst, however, is not to encourage agreement regarding the familiar, but to move us beyond what is currently fashionable or given in our descriptive or moral categories. On this score, as an analysis rather than an appeal, the book misses the mark. The big question is not "Whether the value judgment which motivates this book deprives it of objectivity" (p. 17)—not value commitment but analytical pre-commitment is the source of its weakness.

Still, if this small book does not handle the large problems involved in any encounter with alienation, it clearly reflects them and it forces



the reader to discern more sharply what these problems are. My own current accounting of these problems, based upon what is both explicit and implicit in this book, can be put in the form of six questions: (1) To what extent is alienation—as detachment, aloofness, neutrality—a fundamental feature of the very *philosophy of science* that is employed to investigate it? (2) What is the best way to develop a more coherent *conceptual order* in this area where disorder is a prominent feature? (3) What is the *theory of self* that underlies the depiction of people who are not “spontaneous,” who find it “hard to be themselves,” and who are shaped by forces with which they have “no inner relationship?” (4) What is the *moral ground* on which we can defend given choices as instances of an “authentic” life—a life which is genuine and intrinsic rather than alienated? (5) What is the *historical ground* for imputations about an increasing trend of alienation? (6) What are the alternative *possibilities for solution*, beyond a cursory appeal to the need for radically altered social and economic institutions?

A formidable list, indeed; but that is what makes the theme of alienation an important one.

MELVIN SEEMAN

*University of California, Los Angeles*

*Rise and Development of a Totalitarian State.*

By V. CHALUPA. Leiden: H. E. Stenfert Kroese, N. V., 1959. 294 pp. f 15.-

The rise of a Communist regime in Czechoslovakia in 1948 was a shock and also an eye-opener to the Western World. Within less than three years after liberation from Nazi domination this nation, which had been regarded as a stronghold of democracy, fell under the opposite totalitarian regime and was soon drawn into the sphere of Soviet domination. The case of Czechoslovakia perhaps more than any other event of those years revealed the expansionist policy of the USSR and showed with paradigmatic clarity the communist tactics of seizure and consolidation of power. It is from this point of view that Chalupa approaches his subject.

Part II of his book gives in three chapters a detailed and well documented account of the various phases of the political transformation of Czechoslovakia into a satellite of the Soviet Union. This is preceded by a generalizing treatise on totalitarian regimes and their tactics (Part I), that is, by an explanation of the “concept of totalitarianism” (Chapter 1) and an analysis of the structure and tactics of the communist movement in general (Chapter 2). One major merit of the book is the extensive use of primary sources, both Czechoslovakian and Russian.

As to content, the first part could hardly present any startling new findings, in view of the vast literature on the subject. One might also take issue with some of the author's conceptions, for example, that of managerism. This is, however, one of the most precisely written systematic treatises on Communist organization and tactics that I happen to know. The second and main part contains highly valuable and, I suppose, largely new information. But the presentation is not as interesting as one might expect after having read the first two chapters. This is in part a consequence of the author's limited approach. One would like to learn more about the background of the Communist revolution, particularly the socio-economic conditions after liberation from Nazi domination. One would also like to have some information about the ideology, composition, and policies of the various political parties and a more penetrating analysis of the factors which resulted in their inaction or impotence in 1945–48. Furthermore one wonders to what extent the ethnic heterogeneity of the country contributed to the Communists' success (there is no mention of the Sudeten Germans and their expulsion). Finally, the influence of panslavistic tendencies among the Czech and Slovak elites might have been discussed. This extension of the approach beyond a study of tactics would have enhanced the sociological significance of the book.

Within the limitations set by the author it is a valuable contribution to political sociology as well as to contemporary history. Especially interesting is the description of the gradual subjection of the Czechoslovakian Communist Party to the Russian Communist Party.

Of course, a book of this kind, written by a political refugee, cannot be “objective” and is not so intended. However, the author's effort to present the facts in a detached and unemotional way must be acknowledged and it should be recognized that he is not at all uncritical towards the leadership of the anti-communist parties.

RUDOLF HEBERLE

*Louisiana State University*

*The Politics of Mass Society.* By WILLIAM KORNHAUSER. Glencoe, Ill.: Free Press, 1959. 256 pp. \$5.00.

The label “mass society” is nowadays commonly used to designate the major features and trends of Western industrial civilization. It has an unavoidably pejorative ring and is bandied about so freely by people with widely differing complaints about contemporary life that the concept has become vague and protean, often being used almost synonymously with modern industrial society as such. Yet the political and

ideological overtones of the term are rooted in recent history, specifically in the rise of messianic political movements which have weakened or shattered constitutional limits to the exercise of political power and traditional restraints on human conduct. The threat of a triumphant totalitarian movement has become the ultimate horror of our time, all too facily invoked by ideologists of diverse persuasions aiming at lesser targets.

Kornhauser's book is essentially an effort to systematize the large body of historical and political writings on the roots of Fascism, Communism, and minor anti-democratic movements like Poujadism and McCarthyism. Most of the studies of these movements are insufficiently comparative; the more suggestive analyses of crisis-driven masses open to mobilization by power-hungry elites have been developed in connection with particular interpretations of Nazism and Communism. Kornhauser's aim is to extract from this material reliable generalizations about the types of social structure most prone to mass movements, the kinds of historical change giving rise to them, and their distinctive followings distinguishing them from conventional political parties and interest groups.

The first section of the book attempts to pare down the concept of mass society to manageable proportions. A typology of four kinds of society is developed: the communal, the pluralist, the mass, and the totalitarian, each exhibiting different relations between the state and society. Communal society is decentralized but its members are firmly bound to local groups or inclusive corporations, as in Medieval Europe; pluralist society contains numerous competing groups with overlapping memberships; mass society lacks such a network of associations mediating between the state and the smallest primary groups; and under totalitarianism, as Emil Lederer has put it, "the state swallows up society."

This typology is based on several essential distinctions, yet I find it hard to see that it has much value except as a summarizing measure. Kornhauser criticizes writers who have failed to make his distinctions, but his own categories obscure important historical connections. British pluralism, for instance, has roots in feudal ("communal") institutions, just as Soviet totalitarianism is related to Czarist absolutism or, more broadly, to Oriental despotism, a phenomenon Kornhauser fails even to mention. Also, the Soviet regime carried out through purges and terror a "revolution from above" which created an amorphous, manipulable citizenry resembling that which resulted from war and depression in pre-Nazi Germany. Kornhauser is right to insist on the differences between a

centralized totalitarian society and a weak, conflict-ridden democracy, both of which are often called "mass societies," but there are also illuminating similarities between them that he tends to ignore.

The trouble is that "pluralism" and "mass society" are examples *par excellence* of what Blumer has called "sensitizing concepts," that is, they are suggestive rather than definitive and acquire full meaning only when closely fitted to historical particulars. When used primarily to refer to modern political institutions, there are simply too few historical instances to give them much independent analytic power. And if, as Kornhauser concedes, "mass" and "pluralistic" are tendencies or matters of degree, why set up a typology of *societies* at all? Kornhauser regularly imputes traits to mass or pluralist society and then musters an array of evidence from studies of modern Western nations to support his generalizations. His typological procedure has ample justification in the thoughtways of contemporary sociology, but by failing to apply it directly to concrete national social structures he consistently side-steps the more crucial questions it raises.

Is America, for example, a pluralist society? Kornhauser argues, apparently with the United States in mind, that modern societies create new intermediate groups replacing those destroyed by urbanization and industrialization. Yet such new associations as the suburban community or the corporation as a "big happy family" are perhaps not simply negations of the "mass" tendencies inherent in loose, atomized social structures, but stand in a dialectical relation to them. And what is the connection between "mass culture," surely more highly developed in the United States than elsewhere, and "mass society?" Kornhauser does refer to France as an outstanding example of mass society, yet the bitterness of the French struggle between Left and Right has inhibited the growth of mass movements, which, as he contends, characteristically unite people by ties other than the class interests expressed in Left-Right conflicts.

The most successful and original section of the book deals with the changes and crisis situations that have "traumatized" modern societies, evoking mass movements in response. Kornhauser ably shows the untenability of theories that single out either the shift from authoritarian to popular government (the standard conservative case against democracy), urbanization and industrialization, or even depression and defeat in war as causes in themselves of extremist movements. Only when these changes have occurred abruptly, creating sharp discontinuities, have they had such an effect. Old hierarchical

societies like France and Germany suddenly exposed to social leveling and drastic institutional change are more prone to extremist movements than new expanding societies like the United States or "organic" culturally homogeneous nations like Britain and the Scandinavian countries.

The main limitation of *The Politics of Mass Society*, one that it shares with much of contemporary sociology, is that it freezes into a set of static concepts the experience of the recent past just at a time when we have grounds for believing that, at least with regard to the domestic conflicts of Western countries, the terrible realities of recent politics are unlikely to be repeated. We are entitled to believe this only with reference to the West, but Kornhauser fails to discuss the role of mass movements in underdeveloped nations. And in the West the non-political manifestations of mass society—"privatization," cultural mediocrity, the dominance of a complacent hedonistic ethic—may pose new and more subtle threats to the spirit of freedom and democracy, as distinct from the formal institutions embodying them.

Despite the critical tone of this review, I feel obliged to assert emphatically that the book is required reading for all students of the sociology of modern politics.

DENNIS H. WRONG

Brown University

*Suburbia: Its People and Their Politics.* By ROBERT C. WOOD. Boston: Houghton Mifflin Co., 1959. xi, 340 pp. \$4.00.

The increasingly suburban nature of American neighborhoods has been noted by social scientists since at least the 1920s and has produced scare headlines in the middlebrow magazines and the middlebrow paperback press for the past several years. Yet there has been a curious lack of interest in the emerging suburban landscape as a research area. Now, in the last year, we have several major efforts to subject suburbia to intellectual control. Mr. Wood's volume, particularly, indicates both the potential importance of suburbia for social science and our present poverty in theory and information.

Mr. Wood, a political scientist by trade, has certain concurrent jurisdictional rights to this virgin territory; whatever else suburbia is, it is usually defined as politically separated from the core city. In exercising those rights he had produced an image of the suburbs which may be somewhat novel to a sociologist: he concentrates upon the suburb as a form of political group.

His discussion begins with a consideration of ideal local government, as defined by American political culture—"the republic in miniature."

He then deals briefly and pungently with the rise of urban complexes and the problems of urban government in America during the gaslit ages. He sees the development of suburban neighborhoods, politically autonomous and economically dependent, as a result of *flight*, from the government of the metropolis and from the city streets ("going on and on forever, out of nowhere into nothing," as Sherwood Anderson put it.)

He turns then to describe suburbia today, emphasizing the familiar demographic characteristics. He postulates the development of "natural neighborhoods" as a consequence of relative homogeneity and the spatial decentralization of services, noting that they occur within the core city as well as on its edges. Then he emphasizes the effects of political boundaries, political powers, political institutions, in giving a separate social form to each suburb. Suburbs are defined as "natural neighborhoods plus politics."

He then treats of suburban politics, noting the vexed question of changes in political moiety which may or may not occur with suburbanization. His chief emphasis, however, is upon the lack of success the national political parties have had in bringing suburbia within the central city organizations. He relates this to the neutralization and personalization of local politics in the small municipalities, the assumption of consensus among friends and neighbors which is antithetical to fierce partisanship. From the non-partisan but complex daily rounds of suburban governments, he then derives the notion of the City Manager as a neutral boss, with the good government caucus and the League of Women Voters as his ward-healers.

Turning to the public problems of suburbia, he attempts to distinguish between those which may be called "esthetic" (since they seem to be chiefly due to the lack of symmetry and overt common sense in the layout of the governmental boundaries and machinery) and those which reflect a disproportion between resources and the goods that the suburban municipalities have to deliver to stay in business. He makes an important argument, in connection with the latter problems, to the effect that the "new Federalism" of the welfare state may bail out the suburban municipalities—providing them with sufficient support to operate the public schools and streets they cannot afford from their own tax-bases.

Finally he develops a "checklist of operative democracy," emphasizing the rule of law, the role of controversy, and the quest for freedom. By this criterion, suburbia comes out rather poorly in comparison with the large-scale metropolitan government. But Wood concludes that

the suburban governments, *ad hoc* in origin and in constant violation of the laws of public administration, are nevertheless minimally afloat and apt to be with us for a very long time.

Although Mr. Wood is well aware that there are suburbs and suburbs, he continually falls into discussing an ideal-type of suburb (somewhere between Crestwood Heights and Park Forest)—and sometimes endows this ideal-type with a corporate personality which allows it to act, react, plan, and worry. The reason for this use of the ideal-type is simple: his treatment suffers from sheer lack of data. The nature of the political process of suburbia, its relation to the social structure, and the relative importance of demographic factors are assumed, not demonstrated. Indeed, they can not be demonstrated at present. Plausible as they are, his notions are extremely hypothetical in status. Nor can he document the dynamics of suburban development; certainly the specific mechanisms of residential mobility demand investigation if the "flight to the suburbs" is entertained as a serious hypothesis. More basic for his argument is the nature of the events which brought the annexation of outlying areas by the central city to a grinding halt sometime during the early 1920s. Very little light is shed upon these events.

However, Mr. Wood can be sharply criticized only for rhetoric; checking his sources and determining the evidential support for his statements, we emerge with a lively, sophisticated, semi-systematic set of special theories about mid-20th century suburbs. Most striking, for a sociologist, will be his emphasis upon inherited organizational forms as determinate (in an area where we tend to use the model of a free market in space), and determinate precisely because they have the powers of legal coercion. Certainly such an approach can generate a set of research hypotheses worth testing.

SCOTT GREER

Northwestern University

*Legislative Behavior: A Reader in Theory and Research.* Edited by JOHN C. WAHLKE and HEINZ EULAU. Glencoe, Ill.: Free Press, 1959. ix, 413 pp. \$7.50.

Political scientists agree that legislative bodies have undergone a secular decline in the Twentieth Century as policy deliberation has become increasingly the inescapable result of government administration. Executive and bureaucratic arrogation of such initiative, whether willful or not, has altered the policy-making role of legislators in the most fundamental ways. At the same time, the representative role of legislators has been changed profoundly, as Otto Hork-

heimer has shown, with the shift in the structure of political compromise from the problem of individuals and their property to the problem of relations among groups and associations in mass democracy. Whether the eclipse of the parliamentarians as innovators and their frequent transformation from brokers to pawns signal a demotion to the "middle levels of power," as C. Wright Mills argues of the U. S. Congress, by no means follows conclusively. Substantial influence in matters of war, peace, supply, and what Burke called "gaming" in the domestic economy remain.

In the twilight of legislative supremacy in modern government, understanding—like the flight of Minerva's owl—comes late. Which is not to cast doubt but to celebrate the appearance of this collection of 42 essays, most of them reporting recent research on various aspects of legislative behavior throughout the Western world. Wahlke and Eulau have gathered a wide variety of fugitive articles, mostly published earlier in political science journals, and have added four new essays: Derge on urban-rural voting in a state legislature, Freeman on city council voting alignments, Rosenau on Senate attitudes toward Secretary of State Acheson, and Riker on a technique for judging the significance of roll call votes. In brief introductory notes, the editors discuss four main parameters of legislative behavior—institutional, political, social, and psychological—which serve to organize their material. Finally, they present seven applications of techniques for the study of legislative behavior, including game theory, measures of cohesion, factor analysis, and scale analysis. The result is not only a serviceable reference work but an invitation to reflect upon the current direction of theoretical endeavor in this area of political sociology, one in which, incidentally, political scientists have shown the most interest.

If there is any major deficiency in this volume, it is the consequence of the editors' implicit assumption that there is some necessary significance and equivalence in what legislators do throughout the world. Their introduction begins with an arguable proposition: "Of all political institutions, none is more vital to the process of linking governors and governed in relationships of authority, responsibility, and legitimacy, than the modern legislature" (p. 3). This belief, while perhaps necessary to justify the arrangement of much of their material along static lines of structural-functional analysis, also complicates one of the most noteworthy features of their collection. For by including articles on behavior in the legislatures of Australia, Britain, France, Germany, and the United States, among



other nations, the editors believe they "will help promote that broad view of the legislative process which, in turn, will eventually lead to genuinely comparative analyses of legislative behavior" (p. 4). Surely, with substantial differences in the structures of "authority, responsibility, and legitimacy" relationships among these nations, the position of legislatures within national power structures varies rather widely.

Yet their focus upon systematic role theory as the heart of the theoretical endeavor, if one may borrow an image, assumes the comparability of "figures" detached from different "grounds." The editors write: "A viable conceptual model of legislative behavior which is both theory-oriented and empirically relevant might make two assumptions: first, that the legislature, as an institution, is a sub-system of action in the larger political system; and, second, that it is linked with other political sub-systems—such as the party system, the system of pressure groups, or the system of constituencies—by the roles which legislators take in various sub-systems, including the legislature itself" (p. 356).

If individual political cultures foster idiosyncratic legislative behavior (as explored in Samuel H. Beer's excellent essay on "Pressure Groups and Parties in Britain" in this volume), and if legislative power is declining at uneven rates among the Western democracies, perhaps Wahlke and Eulau have begun with much too narrow a conception of political relevance. This suspicion is strengthened by the difficulties encountered even in comparative research on American state legislatures, some of which is reported in this book. The problem is how to bring under control additional dimensions of context and change which give different meanings to legislative role behaviors in various places, instead of assuming an unwarranted vitality among the parliamentarians of the world.

KENNETH G. OLSON

Smith College

*Persuasion: How Opinions and Attitudes Are Changed.* By HERBERT I. ABELSON. New York: Springer Publishing Co., 1959. ix, 118 pp. \$3.75.

The author sets for himself the difficult task of presenting the gist of scientific research on persuasion or opinion and attitude change in a form and language suitable for readers not trained as social scientists. Just who the audience might be is unspecified, but apparently it is people engaged in persuasion, who therefore might want to apply such scientific information

in their roles as advertisers, public relations officers, communicators, and the like.

The book's virtues for this audience, in turn, limit its usefulness for the social scientist. For example, the brief treatment given to each study provides insufficient detail about methodology and findings to permit the reader to evaluate the results, either alone or in comparison with other studies. On the other hand, the author carefully cites each study used and therefore the book may serve a useful bibliographical function, especially directing attention to psychological research on the subject (sociological studies are less adequately covered).

This reviewer finds unfortunate the occasional introduction of chapters by such captions as "How can you best use fear to influence people?" and "Should the audience be told exactly which side you are on?" Such phrasings detract from the generally neutral, objective, and cautious treatment which the author otherwise employs in his description of studies, findings, and principles of opinion and attitude change. At worst, such formulations may stimulate the public's fear of social scientists as manipulators of human lives.

Persuasion is treated in six chapters, respectively devoted to how to present issues, the influence of groups, the persistence of opinion change, the audience as individuals, the persuader, and broad issues related to the study of persuasion. There is a brief chapter on social science methods, another on definitions, and a list of references.

Transmitting the sense, strength, and limitations of social science research to the lay public is, as noted, a difficult task. In this work Dr. Abelson has made a commendable start.

CHARLES R. WRIGHT

University of California, Los Angeles

*The Weapon on the Wall: Rethinking Psychological Warfare.* By MURRAY DYER. Baltimore: The Johns Hopkins Press, 1959. xxi, 269 pp. \$6.00.

This volume is an effort to appraise the past accomplishments and potentialities of international mass communications in support of United States foreign policy. As such, it is more thoughtful and more realistic than most "think pieces" on the topic of "psychological warfare." The basic thesis is that the United States has not fully exploited this instrument of national policy. The book is of interest to sociologists because it represents a conscious effort to assemble social science research to deal with a most complex political and organizational problem.

The author, a former teacher of English, and

the suburban governments, *ad hoc* in origin and in constant violation of the laws of public administration, are nevertheless minimally afloat and apt to be with us for a very long time.

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Political scientists agree that legislative bodies have undergone a secular decline in the Twentieth Century as policy deliberation has become increasingly the inescapable result of government administration. Executive and bureaucratic arrogation of such initiative, whether willful or not, has altered the policy-making role of legislators in the most fundamental ways. At the same time, the representative role of legislators has been changed profoundly, as Otto Hork-

heimer has shown, with the shift in the structure of political compromise from the problem of individuals and their property to the problem of relations among groups and associations in mass democracy. Whether the eclipse of the parliamentarians as innovators and their frequent transformation from brokers to pawns signal a demotion to the "middle levels of power," as C. Wright Mills argues of the U. S. Congress, by no means follows conclusively. Substantial influence in matters of war, peace, supply, and what Burke called "gaming" in the domestic economy remain.

In the twilight of legislative supremacy in modern government, understanding—like the flight of Minerva's owl—comes late. Which is not to cast doubt but to celebrate the appearance of this collection of 42 essays, most of them reporting recent research on various aspects of legislative behavior throughout the Western world. Wahlke and Eulau have gathered a wide variety of fugitive articles, mostly published earlier in political science journals, and have added four new essays: Derge on urban-rural voting in a state legislature, Freeman on city council voting alignments, Rosenau on Senate attitudes toward Secretary of State Acheson, and Riker on a technique for judging the significance of roll call votes. In brief introductory notes, the editors discuss four main parameters of legislative behavior—institutional, political, social, and psychological—which serve to organize their material. Finally, they present seven applications of techniques for the study of legislative behavior, including game theory, measures of cohesion, factor analysis, and scale analysis. The result is not only a serviceable reference work but an invitation to reflect upon the current direction of theoretical endeavor in this area of political sociology, one in which, incidentally, political scientists have shown the most interest.

If there is any major deficiency in this volume, it is the consequence of the editors' implicit assumption that there is some necessary significance and equivalence in what legislators do throughout the world. Their introduction begins with an arguable proposition: "Of all political institutions, none is more vital to the process of linking governors and governed in relationships of authority, responsibility, and legitimacy, than the modern legislature" (p. 3). This belief, while perhaps necessary to justify the arrangement of much of their material along static lines of structural-functional analysis, also complicates one of the most noteworthy features of their collection. For by including articles on behavior in the legislatures of Australia, Britain, France, Germany, and the United States, among

other nations, the editors believe they "will help promote that broad view of the legislative process which, in turn, will eventually lead to genuinely comparative analyses of legislative behavior" (p. 4). Surely, with substantial differences in the structures of "authority, responsibility, and legitimacy" relationships among these nations, the position of legislatures within national power structures varies rather widely.

Yet their focus upon systematic role theory as the heart of the theoretical endeavor, if one may borrow an image, assumes the comparability of "figures" detached from different "grounds." The editors write: "A viable conceptual model of legislative behavior which is both theory-oriented and empirically relevant might make two assumptions: first, that the legislature, as an institution, is a sub-system of action in the larger political system; and, second, that it is linked with other political sub-systems—such as the party system, the system of pressure groups, or the system of constituencies—by the roles which legislators take in various sub-systems, including the legislature itself" (p. 356).

If individual political cultures foster idiosyncratic legislative behavior (as explored in Samuel H. Beer's excellent essay on "Pressure Groups and Parties in Britain" in this volume), and if legislative power is declining at uneven rates among the Western democracies, perhaps Wahlke and Eulau have begun with much too narrow a conception of political relevance. This suspicion is strengthened by the difficulties encountered even in comparative research on American state legislatures, some of which is reported in this book. The problem is how to bring under control additional dimensions of context and change which give different meanings to legislative role behaviors in various places, instead of assuming an unwarranted vitality among the parliamentarians of the world.

KENNETH G. OLSON

Smith College

*Persuasion: How Opinions and Attitudes Are Changed.* By HERBERT I. ABELSON. New York: Springer Publishing Co., 1959. ix, 118 pp. \$3.75.

The author sets for himself the difficult task of presenting the gist of scientific research on persuasion or opinion and attitude change in a form and language suitable for readers not trained as social scientists. Just who the audience might be is unspecified, but apparently it is people engaged in persuasion, who therefore might want to apply such scientific information

in their roles as advertisers, public relations officers, communicators, and the like.

The book's virtues for this audience, in turn, limit its usefulness for the social scientist. For example, the brief treatment given to each study provides insufficient detail about methodology and findings to permit the reader to evaluate the results, either alone or in comparison with other studies. On the other hand, the author carefully cites each study used and therefore the book may serve a useful bibliographical function, especially directing attention to psychological research on the subject (sociological studies are less adequately covered).

This reviewer finds unfortunate the occasional introduction of chapters by such captions as "How can you best use fear to influence people?" and "Should the audience be told exactly which side you are on?" Such phrasings detract from the generally neutral, objective, and cautious treatment which the author otherwise employs in his description of studies, findings, and principles of opinion and attitude change. At worst, such formulations may stimulate the public's fear of social scientists as manipulators of human lives.

Persuasion is treated in six chapters, respectively devoted to how to present issues, the influence of groups, the persistence of opinion change, the audience as individuals, the persuader, and broad issues related to the study of persuasion. There is a brief chapter on social science methods, another on definitions, and a list of references.

Transmitting the sense, strength, and limitations of social science research to the lay public is, as noted, a difficult task. In this work Dr. Abelson has made a commendable start.

CHARLES R. WRIGHT

University of California, Los Angeles

*The Weapon on the Wall: Rethinking Psychological Warfare.* By MURRAY DYER. Baltimore: The Johns Hopkins Press, 1959. xxi, 269 pp. \$6.00.

This volume is an effort to appraise the past accomplishments and potentialities of international mass communications in support of United States foreign policy. As such, it is more thoughtful and more realistic than most "think pieces" on the topic of "psychological warfare." The basic thesis is that the United States has not fully exploited this instrument of national policy. The book is of interest to sociologists because it represents a conscious effort to assemble social science research to deal with a most complex political and organizational problem.

The author, a former teacher of English, and

one time employee of the Office of War Information, is currently a staff member of The Operations Research Office, a contract research agency of the Department of the Army. He seeks to break out of what he considers to have been an overly narrow concept of psychological warfare, which he believes was fostered, in good part, by the military establishment. While this is a laudable objective, he is fighting historical windmills. The "grand strategy" of U. S. world-wide propaganda both during wartime and in the cold war period has been firmly in the hands of civilian political leaders. The strength and weakness of our international information efforts have been rooted in our domestic social and political system which have set the limits on our foreign policy. Because the author does not come to grips with these fundamentals, his analysis remains incomplete. Despite the vast flow of writings on foreign policy, the internal social and political sources of American behavior in international relations remains a neglected topic.

Dyer seeks to redefine psychological warfare as "political communications," and to this end he draws on the writings of Leonard Cottrell, Jr. Such a redefinition, aside from its correct semantics, would have the advantage of eliminating the exaggerated claims put forth by academic psychologists and advertising personnel. But he is fully aware that more than a change in terminology is required. He understands that the success of United States information services depends on U. S. political, economic, and military policies. The best portions of the volume deal specifically with the tasks of relating international information to governmental policy.

Nevertheless, a central focus of the book is on problems of organization, especially on lines of formal authority and clarity of administrative responsibility. His recommendations generally are for greater centralization, although such recommendations might have the consequences of standardizing present activities rather than increasing flexibility and creativity. In my opinion, U. S. information services—both civilian and military—suffer, to the contrary, from a lack of initiative at the middle and lower levels. Increased centralization will not overcome these defects. Clarity of direction will come from the development of common doctrine and from positive civilian leadership, rather than merely from altering organizational charts. This is not to deny that there are real organizational problems, especially those of recruiting and retaining first-rate personnel in these government services.

MORRIS JANOWITZ

University of Michigan

*Propaganda Analysis: A Study of Inferences Made From Nazi Propaganda in World War II.* By ALEXANDER L. GEORGE. Evanston, Ill.: Row, Peterson & Co., 1959. xxii, 287 pp. \$6.00.

The story of the study of propaganda analysis is much the same in recent decades as that of other types of research in social psychology, political science, and sociology. Until the start of World War II, students of propaganda analysis mostly tried to apply research procedures to develop for the citizenry at large a better comprehension of what propagandists try to do with us. Such students viewed their subject, as many of the unorthodox still do, as a way to contribute to liberal education in our colleges and among adults more generally, and thus to the welfare of our democratic institutions.

During World War II, "success" came to propaganda analysts and to other social scientists. They found that they could be important and certainly better rewarded if they would develop their methods as instruments more specifically useful to manipulate publics rather than to help intelligent citizens guard against manipulation. Thus they put aside ideas of objectivity and service to humanity which they may have learned in some old-fashioned graduate school. They discovered the security of neatly punched cards and the marketability of precise columns of figures. They put on the intellectual livery of one or another unit of government, business, labor, or whatnot and became happy technicians for management.

George's study is "part of the research program undertaken for the U. S. Air Force by The RAND Corporation . . . some years ago." The University of Chicago later accepted the report as a Ph.D. dissertation in political science. George explores problems associated with making inferences from analyses of enemy propaganda in war-time. He also tries to check the accuracy of such inferences from German propaganda made for the Federal Communications Commission. He does so against data subsequently obtained from German war records and from interviews with German officials.

As in a great many other such investigations, despite the availability of thoughtful social scientific findings to the contrary, both the FCC and George apparently began with a largely quantitative, content-analysis approach. Then the FCC is said to have learned that "it is often difficult, sometimes imprudent, and sometimes unnecessary to utilize systematic quantitative procedures" (p. 83).

A long and difficult road lies between the study of content as such and the study of content as a tangle of symptoms concerning ongoing processes. One needs to push toward re-



lating content to characteristics of the propaganda's sponsor, technicians, communications media, organizational media, fantasied publics, actual publics, feed-back, and historical and contemporary social contexts. The FCC traveled part of this road, and George probed along it somewhat further. Neither apparently comprehended fully the highly relative and processual character of content data. They learned that propagandists care nothing for consistency as such and prize a flexibility—even a fluidity—of operation that is most destructive to carefully developed punchcard categories. Even though 81 in 100 of the FCC inferences which George could check are said to have been technically verified, the significance of this proportion is not clear. Other inferences could not be checked. The inferences compared with German data were ones on which the German data were of an apparently adequate and dependable sort; the inferences are not a statistical sample of all inferences drawn by the FCC staff. The probable significance of inaccurate and unverifiable inferences could be crucial.

ALFRED MCCLUNG LEE

Brooklyn College

*The Ineffective Soldier: Breakdown and Recovery.* By ELI GINZBERG, JOHN B. MINER, JAMES K. ANDERSON, SOL W. GINSBURG and JOHN L. HERMA. Foreword by HOWARD MCC. SNYDER. New York: Columbia University Press, 1959. xvii, 248 pp. \$6.00.

*The Ineffective Soldier: The Lost Divisions.* By ELI GINZBERG, JAMES K. ANDERSON, SOL W. GINSBURG and JOHN L. HERMA. Foreword by HOWARD MCC. SNYDER. New York: Columbia University Press, 1959. xx, 225 pp. \$6.00.

*The Ineffective Soldier: Patterns of Performance.* By ELI GINZBERG, JAMES K. ANDERSON, SOL W. GINSBURG and JOHN L. HERMA. Assisted by DOUGLAS W. BRAY, WILLIAM A. JORDAN, and FRANCIS J. RYAN. Foreword by HOWARD MCC. SNYDER. New York: Columbia University Press, 1959. xix, 340 pp. \$6.00.

One of the major "applications" of social science research has been the rapid proliferation and widespread acceptance of psychiatric and psychological personnel testing and selection. These devices are used not because of their demonstrated validity and predictive power but, in part, because large scale organizations must have fixed and general criteria for recruitment and selection.

Perhaps the most extensive and dramatic use of these procedures was by the armed forces during World War II. Military managers were faced with the necessity of mobilizing and

screening rapidly some eighteen million men. Many would be given a combat assignment, a task which had no or little counterpart in civilian life. The three-volume study, *The Ineffective Soldier*, is a massive and careful documentation of the failure of this screening system. The basic finding is that except for rejecting extreme deviants—the psychotic syndrome—the selection system failed to predict effectively. In fact, by screening out large numbers of potentially usable personnel, the armed forces were deprived of valuable human resources. The lesson for civilian administrators is that personnel management requires more than selection; it rests on comprehensive procedures of training, assignment, and supervision.

The research was carried out over an eight-year period by the Conservation of Human Resources Project at Columbia University under the directorship of Professor Eli Ginzberg, a manpower economist. The group had the full cooperation of the armed forces and of the Selective Service system. As a result, these volumes represent a neat balance between hard data and sophisticated interpretations. Unfortunately, the style of reporting is repetitious and at points needlessly overexpanded.

The approach of the authors is broad and fundamentally "sociological" in the best sense of the word. Performance of the soldier is a function of his personal characteristics, the situational stress under which he finds himself, and administrative practices of the organization. Personnel selection is inherently limited because at best it measures only one factor in the equation—namely, personal characteristics.

The data deal with the records of the Army, that is both the ground and the air forces from 1941 to 1945. The research design focuses on the interplay between the screening procedures and premature separation from active service. The reasons for separation were classified as psychoneurosis, psychosis, undesirable habits and inaptitude (including illiterates). Each volume represents a different but converging research approach. Volume I is based mainly on an ingenious analysis of official statistics and records. Volume II is made up of a series of case studies which include not only patterns of breakdown but also post-release efforts at rehabilitation. While these case materials are vivid they add very little to the central argument. The most important and original aspects of the study are presented in Volume III, which presents a detailed examination of the service records of a cohort sample of some 72,000 who entered military service in 1942 and who were prematurely separated.

The rejection rate in World War II was found

to be about eleven times as great as in World War I for men with emotional defects, and four and one-half times greater for men with mental defects. Unnecessarily rigid standards were set because the Army insisted that all men be convertible into infantry men though no more than two out of five service men would eventually serve in ground combat units. Only late in the war did the Army succeed in developing a test which could discriminate fairly well between a man's education and background and his ability to learn and apply what he could be taught, although such a test was crucial for selecting out the truly inapt. While the Army did start training programs for illiterates and those without adequate fundamental education, large numbers of usable men were arbitrarily screened out. More important, the Army never did succeed in developing psychological and psychiatric criteria which could reliably differentiate among selectees as to their future emotional stability. Thus, the authors conclude that had the Army concentrated on rejecting only the seriously disturbed, it would have rejected fewer men who could have served, and probably would have accepted fewer of those who eventually failed.

In the end, the whole procedure was modified. The standards had to be revised, and the Army found itself with a serious shortage of infantry reserves. The authors are fully aware of the severe limitations in personnel and the pressure of time under which the screening system worked. But their conclusions underline the inherent and basic weakness of the whole procedure. They are careful but yet pointed in criticizing the psychiatric profession for encouraging military officials to believe that valid procedures were available to separate the "strong from the weak, the reliable from the unreliable on the basis of a quick psychiatric evaluation."

But the failure of the manpower practices in World War II was more than a limitation in screening procedures, important though they may have been. Manpower practices ran into difficulty because of the strategic decisions which under-allocated manpower in numbers and quality to the ground as compared with the air forces and naval forces. Why the error, which involved countless specific decisions, was made remains a crucial question for students of organizational behavior. Was it an unavoidable miscalculation, because military planners had no rational basis on which to make a more accurate decision? Was it the outcome of civilian perspectives which in the United States have traditionally seen military operations as jobs for machines rather than for "heroic fighters"? Even in the absence of personnel screening procedures the same allo-

cation of resources might well have been made.

*The Ineffective Soldier* has set the stage for a continuing investigation of the administrative and sociological consequences of personnel selection procedures. The vast machinery of selecting students for higher education requires such a continuing and sophisticated audit. But the authors seem to me to be unduly pessimistic about the appropriate relevance of psychological and psychiatric testing in administrative organization. In one sense, these war time experiences are not a comprehensive test because selection for combat is such a unique and specialized problem. There is, as they indicate, little basis for using such procedures to recruit "leaders;" in fact, there is great danger if they are used for this purpose that they may have the effect of selecting out truly creative personnel. Nor is there much hope that personality research has been or will be in the near future perfected to the point of permitting positive matching of specific desirable traits to specific occupations.

Clearly, their greatest potential rests in excluding highly deviant personalities from selected stressful occupations. But the negative screening may have greater possibilities. It seems likely that certain types of organizations can determine which patterns of personality traits are completely undesirable for their needs. For example, in the selection of personnel to work in children's institutions or in corrections schools, we already know which personality interests should be re-assigned to other tasks.

Moreover, British experiences and some American work, such as reported by George G. Stern, Morris Stein, and Benjamin Bloom, which employ quasi-experimental group situations, group observations, and more wholistic evaluations by multiple observers may be more relevant than the individual brief psychiatric examinations or paper-and-pencil tests. The most useful work seems to be done where psychological and psychiatric testing are incorporated into traditional personnel procedures and information pooled rather than used independently as a final yardstick. While all these procedures need to be "cut down to size," it would be unfortunate if the present healthy reaction to exaggerated claims prevented further explorations in personality assessment for administrative purposes. This seems most unlikely, given the large number of persons who are energetically committed to the work. What is required is a greater understanding of the organizational realities in which their tests must operate, and for this purpose the Ginzberg study is an important but not typical case study.

MORRIS JANOWITZ

University of Michigan

*My Name Is Legion: Foundations for a Theory of Man in Relation to Culture.* By ALEXANDER H. LEIGHTON. Vol. I. The Stirling County Study of Psychiatric Disorder and Sociocultural Environment. New York: Basic Books, 1959. xii, 452 pp. \$7.50.

The title and subtitle of this book are condensed statements of two facets of the problem of mental illnesses in human society. The title is from a sentence in the New Testament:

*And always, night and day, he was in the mountains and in the tombs crying, and cutting himself with stones. . . .*

*And Jesus asked him, What is thy name? And he answered, saying, My name is Legion: for we are many.*

St. Mark, 5:5, 9

The issue symbolized in the subtitle revolves around the question of how to bridge the theoretical gulf that exists between the way medical men think about psychiatric disorders and the way social scientists think about them. Briefly, the differences in the two thought-streams under discussion may be characterized in the following terms. Physicians tend to focus their attention upon man as an organism and give little attention to the sociocultural environment in which he lives. Social scientists focus their attention upon society and culture and ignore man as the creator and bearer of the sociocultural phenomena they study. As a consequence, two parallel bodies of theory have been developed about man as animal and man as person. This dichotomy is pertinent particularly when one faces problems associated with the etiology of mental illnesses.

The theoretical frame developed in *My Name Is Legion* was essential before Leighton could answer the epidemiological questions he asked when he began to study mental illnesses in the population of a county in Nova Scotia, Canada. The Stirling County Study is concerned with three questions: (1) How much psychiatric disorder exists in the population under study? (2) What disorders are found in the afflicted persons? (3) Are these disorders related to sociocultural factors prevailing in Stirling County? These questions are not answered in this volume. Leighton indicates they will be in the two remaining volumes that are scheduled to be published on this extensive, expensive, and time-consuming piece of research. (The study began in the late 1940s.) The present volume is a statement of a frame of reference that will be followed by the analysis and interpretation of empirical data gathered in Stirling County.

The particular focus of *My Name Is Legion* is the presentation of concepts and propositions which are designed to show how psychopathol-

ogy in an individual may be related to factors in the individual's sociocultural environment. The concepts and propositions are illustrated by concrete descriptive data from Stirling County. None of the concepts is tested by the use of empirical data according to the procedures of social scientists. Leighton states this limitation on several occasions. Presumably the concepts and propositions developed in this book will be put to the test in the succeeding volumes.

Leighton's frame of reference begins with the clinical case. Early in the book he discusses salient features of the mental illnesses of several residents of Stirling County. The orientation of the psychiatrist is presented in the discussion of these cases. Then the central issue of how psychiatric disorders may be linked to factors in the sociocultural environment is presented systematically. A series of propositions which tie the individual to his society is developed.

The concept of *sentiment* is the central focus in Leighton's analytical scheme. The *sentiments* individuals acquire during the course of their lives are the links which bind man as organism and man as person in a sociocultural environment into a functional system. The explication of the concept of *sentiment* takes up a large part of the book. The last third is given over to a statement of how the conceptual frame developed in the first two parts can be applied to the study of mental illness in a community.

An adequate statement of Leighton's theoretical frame cannot be presented here. To do the book justice a long expository review is indicated. For present purposes, this reviewer would like to state that he believes this book will become a classic in this field. However, there are statements in it that practically every sociologist will question, such as Leighton's treatment of the community as a pseudo-organism and his view that the nuclear family is a biological entity. Such statements will be refined by acerbic critics in due course. Psychiatrists are not expected to accept the book without a plethora of reservations. Nevertheless, the book is a promontory on a coast that needs to be explored and carefully mapped.

This book ought to be read by every psychiatrist and social scientist interested in the epidemiology of mental illnesses. The cogency of the theoretical frame, however, to the three questions under search—to wit, how much mental illness, of what kinds, and where in Stirling County—can be assessed only after the next two volumes have been published. But as a foundation for the bridge that needs to be built it shows solid strength.

A. B. HOLLINGSHEAD

Yale University

*The Mother-Child Interaction in Psychosomatic Disorders.* By ANN M. GARNER and CHARLES WENAR. Urbana: University of Illinois Press, 1959. vii, 290 pp. \$6.00.

Sociologists who like to ridicule socialization studies should avoid this book. It is built upon a theory relating psychosomatic disorders of childhood (asthma, rheumatoid arthritis, ulcerative colitis, peptic ulcer, and atopic eczema) to the inability of a mother to get gratification from the bodily care of her infant child. The authors believed that due to the earliness of the mother-child trauma, psychosomatic children will be more mistrustful of intimacy and have greater doubts about being loved than will neurotic children, whose trauma in mother-child relations occurs later in socialization (and does not include hospitalization). Chronically ill children who do not have psychosomatic diseases and who do not necessarily have traumatic mother-child relations, beyond those incident to hospitalization, constitute a second comparison category. With exhausting thoroughness the authors permit the reader to share the confusion of many well-intentioned assessment attempts which failed, but they are still able to draw out evidence to support their essential thesis that psychosomatic children have suffered an earlier and more ineradicable psychological trauma than the neurotic and ill children.

They find consistent and inconsistent mothers of psychosomatic children—a distinction which had not been anticipated in their theory. The first induce dominating, competitive, and angry responses in their children by self-contained and benign behavior and the second induce a helpless and victimized attitude by their competitive and haphazard behavior. The late Dr. Margaret Gerard, whose psychoanalytic approach sparked the original hypothesis, was unfortunately not available to provide a synthesis. Perhaps this explains why the impact of the results on the original theory is so little developed.

The great difficulty in this very important area of research is the need to wait from six to ten years to read the results of early socialization traumas. In the meantime, many other behavior-determining experiences have been accumulated. It is becoming increasingly clear from this study, and from the pioneer work by Sibylle Escalona, Milton Senn, Ernest Kris, and others, that work should start on the study and restudy of a cohort of 1000 mothers and their children each year for the next decade. It is no longer adequate to our needs to reconstruct early training only for the sample of ill or disturbed children, nor should we pretend that the complexities of intervening experience are negligible. The one technical feature which keeps

such a plan from being immediately feasible is our continued clumsiness in describing economically what is to be seen in observable mother-child relations. The present authors' attempt in this direction was clearly inadequate and little improved over Barbara Merrill's efforts of more than twenty years ago. Crude as they are, the findings of this book clearly indicate that there is much of value to be learned from reliable and salient mother-child interbehavioral measures. One can also guess that until these are brought to the precision of pediatric diagnosis, baffling as it is, it may not be possible to answer many of the more central questions raised by this modest and careful study.

FRED L. STRODTBECK

*Center for Advanced Study in the Behavioral Sciences*

*Alcoholism: An Interdisciplinary Approach.* Edited by DAVID J. PITTMAN. Foreword by JOHN C. GLIDEWELL. Springfield, Ill.: Charles C. Thomas, 1959. xviii, 96 pp. \$3.75.

*Drinking Behavior in Small Groups: An Experimental Study.* By KETIL BRUUN. Translated by FRED A. FEWSTER. The Finnish Foundation for Alcohol Studies. Stockholm: Almqvist & Wiksell, 1959. 132 pp. Sw. kr. 16:—, paper.

The first of these books reports the proceedings of a conference on the etiology of alcoholism held in March, 1959, at Washington University in St. Louis. The papers in the first part of the volume were prepared for the conference and summarize findings from physiology, psychology, psychiatry, and sociology, while those in the second part were based on the conference discussions and are concerned with suggestions for future alcoholism research. There appears to be general agreement among the contributors that little has been demonstrated conclusively concerning the etiology of alcoholism. For example, attempts to delineate the alcoholic personality have been uniformly unsuccessful, there is no definitive evidence that alcoholism is actually a clinical entity, and there is still no conclusive answer to the question of whether different individuals have different thresholds of tolerance for alcohol consumption. This slender volume constitutes an excellent summary of the present status of alcoholism research in most of the relevant disciplines, and provides a number of interesting and useful leads for future research in this problem area.

Bruun's monograph describes a study in which fifteen four-person groups were assembled for six-hour drinking sessions in order to investigate a series of hypothesized relationships between group drinking and certain attitudinal and behavioral variables. The observers collected data



on actual drinking behavior, attitudes toward acceptance of drinking and of drunken aggression, changes in quantity of interaction and in proportions of positive and negative emotional reactions according to role differentiation within the group, and changes in group cohesion and stability. The author concludes that at least some of the behavioral changes, namely, the changes in the proportion of positive and negative emotional interactions, are due to the consumption of alcohol. He points out, however, that the amount of change cannot be explained by the amount of alcohol consumed. Moreover, he concedes that the consumption of alcohol is not a necessary condition in that similar changes may occur in group situations which offer functional alternatives to the drinking situation.

OZZIE G. SIMMONS

Harvard University

*Predicting Delinquency and Crime.* By SHELDON and ELEANOR GLUECK. Cambridge, Mass.: Harvard University Press, 1959. xxii, 283 pp. \$6.50.

The Gluecks have toiled in the field of criminal prediction since the late nineteen-twenties when they constructed their first prediction table from the experiences of 500 male prisoners. This synoptic volume includes that first table along with later ones based on the experiences of analogous but less renowned cohorts of offenders. While one may wonder about the value of these actuarial studies, one cannot but admire the persistence of the Gluecks on a path made up so largely of obstacles. Nor can one but respect their frank admissions concerning the tentative nature of their findings which, as they frequently declare, stand in need of validation.

The Gluecks are of the firm opinion that prediction tables can assist judges and other penal officers in choosing wisely among alternatives, such as parole, imprisonment, and probation. To that end, they have appended their "network" of 60 tables, each giving selected score intervals and the associated odds of success. Of course, in the absence of empirical checks it is impossible to determine whether such tables would indeed rationalize the decisions of penal courts and boards. At present one is inclined to be skeptical, since most actuarial tables of this kind have not proven themselves to be more efficient than predictions from the overall success rates. In any event, the Gluecks offer their tables more as an aid to the practitioner than as a contribution to theory.

Sociologists will find themselves agreeing and disagreeing with the viewpoints of the Gluecks, and disagreeing among themselves in regard to what is significant. For instance, many will be

inclined to reject the view that causal knowledge is established through the technique of prediction, and that speculation regarding an "abstract unitary causal theory" is idle and useless. Essentially, this is the old issue of induction versus deduction, and the Gluecks happen to believe in the former. And for those who prefer the latter, they presumably would welcome any attempt to order their diverse facts by a single self-contained theory. The theory of differential association lends itself to such an experiment, since many of the Gluecks' findings appear to be readily subsumed under that theory.

The mild attempt to discredit the significance of the neighborhood as a factor in crime is also likely to unsettle some sociological readers. The Gluecks note (p. 45) that place of residence is not predictive of parole success in Chicago, "despite the emphasis of certain sociological criminologists" on that factor. *A priori* one would expect residence to be impotent to predict success on parole in any large city, for the reason that most convicts are paroled to the same area. But this does not nullify the importance of neighborhood as a factor in producing crime.

But as often as not, the Gluecks dwell on the relative importance of sociological influences. For example, they attach greater importance to the pattern of interpersonal relations within the family than to any other single circumstance; they repeatedly note that the structure of parental behavior is more prognostic of delinquent conduct than any other factor. Again, they point out (p. 109) that of 77 sociocultural traits, 36 were found to differentiate between the treatable and untreatable offenders; whereas of 74 traits drawn from psychological tests, only one trait bore a significant relation to outcome. Judging from these studies together, it appears that social factors are potentially as predictive of human adjustment as individual factors.

Methodologically, this volume presents nothing new, but then it makes no pretensions to advance this subject. While the Gluecks do not disparage post-war efforts to solve various technical problems, they feel the burden of proof is on those who occupy themselves with such issues. They comment: "it remains for the proponents of more sophisticated predictive devices to demonstrate pragmatically that the application of various refinements does in fact markedly improve the practical utility of predictive instruments" (p. 150).

One "prediction" table calls for critical notice: that which codifies the experiences of approximately 500 delinquents and 500 controls (p. 233). The Gluecks interpret the percentage of delinquent boys in each score category as the

probability that a randomly selected boy will become delinquent. But this would be valid only if the percentage of delinquents and non-delinquents in the general population were equal. Otherwise, the score distribution of non-delinquents would have to be weighted according to their relative strength in the general population. This adjustment would undoubtedly reduce the differentiation between delinquents and non-delinquents and thereby lower the predictive efficiency of their table.

Altogether, the chief value of this book is as a plain exhibition of the vexatious problems that confront anyone seeking to predict human adjustment. Most vexing is the fact that the social conditions under which the predicted adjustment occurs often bear only slight resemblance to the conditions prevailing at the time the experience table was built.

KARL SCHUESSLER

Indiana University

*Adolescent Aggression: A Study of the Influence of Child-Training Practices and Family Interrelations.* By ALBERT BANDURA and RICHARD H. WALTERS. Foreword by ROBERT R. SEARS. New York: Ronald Press, 1959. xii, 475 pp. \$7.50.

Focusing on the familial antecedents and correlates of antisocial aggression, held to be a cleaner variable than juvenile delinquency, this competent and readable monograph represents in many ways a model of theory-oriented research. The authors develop a detailed theory which turns upon the notion that antisocial aggression results from a disruption in the child's dependency relationship with his parents: lack of affectional nurturance and an excess of harsh discipline produces a state of dependency anxiety and this, in turn, functions both to instigate hostility and aggression, and to block parental identification and modeling. Setting forth a series of specific hypotheses designed to represent various empirical implications of this general theory, the authors undertake to test them by comparing a group of twenty-six antisocial aggressive boys with a group of roughly paired non-aggressive controls. Semi-structured interviews with these boys and with each of their parents were rated on some 150 variables, mostly with satisfactory evidences of reliability and validity. In threading an orderly path through this mass of material, the authors follow the "story line" dictated by their theory, presenting for each point *t* tests of mean differences between the aggressive families and the controls, supplemented by vivid excerpts from the recorded interviews. Although not all tests succeed, the great majority of them turn

out rather impressively in the specified directions.

The findings present a familiar and depressing story. In contrast to the controls, parents of the aggressive boys are shown to be less warm, more rejecting, especially of dependent behavior, to make fewer and less consistent demands, and to rely more upon punitive techniques than upon reasoning in their sporadic attempts to enforce them. While the fathers are somewhat more demanding and consistent than the mothers, they have little affectional interaction with their sons, show considerable hostility to them, and nag, scold, and spank more than the control fathers do. As a result, the sons develop little identification with their father, feel hostile toward him, spend little time with him, and avoid seeking his help. Although there is somewhat more affection between these aggressive boys and their mothers, her influence on socialization is generally weak, perhaps because she makes relatively few demands and is inconsistent in carrying through on them. Typically also there is hostility between the parents, with the result that they do not support one another's attempts at discipline. Reacting to the frustration and hostility of the home situation, the sons, in contrast to the controls, are shown to express their aggression against teachers and peers more directly than indirectly, and to feel relief rather than guilt following aggressive episodes. It appears, in fact, that the aggressive boys have very little internalized conscience at all.

This pattern of validated findings does indeed seem to fit the authors' theoretical model rather well, but not so neatly as to exclude alternative interpretations. Indeed, the differences between the aggressive families and the controls are so gross and so numerous, one gains the impression that most any other plausible theory would check out almost equally well: bad boys come from bad families! In this connection, the authors' attempts at secondary analysis, which consist solely of reporting showers of zero-order correlation coefficients among their mass of variables, seem particularly unfortunate: their general effect is not so much to exclude alternative interpretations as to suggest them. But here there may also be an element of artifact: with aggressive and control groups occupying extreme opposite corners on most distributions, pooled data would seem almost certain to yield handsome correlations wherever one looks.

Child psychologists will miss in this study any explicit treatment of infant discipline. While focusing the interviews heavily upon current family relationships avoids some of the pitfalls of retrospective reporting, one must wonder whether the parents who now reject and punish their sons do so because they have turned out

to be, for whatever reasons, so eminently rejectable and deserving of punishment. Sociologists will miss any explicit concern with the usual variables so central in sociologically-oriented studies of delinquency: social class and neighborhood are treated not as variables but as controls; subjects from broken homes, deteriorated neighborhoods, and ethnic minorities are excluded from the study; and problems of peer groups and youth cultures are never considered.

But let us not criticize a monograph for not doing what was clearly outside its field of focus. In terms of what it does do, this reviewer found the study generally competent, convincing, and richly rewarding. In spite of certain reservations on methodological grounds, I believe that until something very much better comes along, this study should stand among the very best we have.

EDWARD C. DEVEREUX, JR.

Cornell University

*The Vanishing Adolescent.* By EDGAR Z. FRIEDENBERG. Boston: Beacon Press, 1959. xv, 144 pp. \$2.95.

Friedenberg, a teacher of social science and education at Brooklyn College, is another commentator who deplores the current emphasis on social adjustment and the "renunciation of individual differences." Adolescence, Friedenberg argues, in the meaningful sense of the concept, is disappearing. The function of adolescence should be self-definition and psychological growth. In "dialectical combat with society" the adolescent clarifies his experience and builds a basis for self-esteem. Adults are obligated to help the adolescent appraise himself and develop his inner resources. But on the contrary, adults in contemporary society, with undue sensitivity and repressed homosexual tendencies, react to the adolescent's individualistic behavior and emotional intensity with fear and hostility. They attack the adolescent's dignity and sense of worth and try to make him conform—the effect of which is to thwart his natural tendencies of development. The adolescent who is not crushed, especially in the lower class, may turn to delinquent groups for self-esteem, a solution basically unsatisfying to himself as well as to society.

The school is the primary target. Teachers have a professional responsibility to promote the healthy development of youngsters. But with no more understanding and personal authority than other adults, they merely affirm the criteria of social acceptance. To promote uniformity and "normality," they restrict the student's freedom, invade his privacy, and limit his enterprise. To support his case, the author cites case material

of his own and refers to such authors as Riesman, H. S. Sullivan, A. Cohen, Hollingshead, and Kahl.

Since the book is intended as an essay, we cannot with fairness criticize the author for so highly valuing individual growth, generalizing so broadly about the "rights" of adolescents and the villainy of adults, or perhaps even for a certain selectivity of available data. However, we can question the author's premise—for which there is little evidence—that the adolescent has a natural pattern of development and the implication that the alleged treatment of adolescents will have such dire effects. If the more or less satisfactory adolescence of the past generation led to such a destructive lot of contemporary adults, could we not make an equally good case for a reversal in the next generation? Friedenberg's ideas are not to be casually dismissed, but they present too simplified and unsubstantiated a picture for social scientists.

Worth noting are many stimulating and provocative ideas incidental to the main argument. To illustrate: the author suggests that we manifest our own disturbances in reacting vindictively to juvenile delinquents; that the "school-teaching career provides the most status in proportion to risk" for upwardly mobile students (p. 78); and that adults, in an oft-repeated reaction pattern, were hostile when rock and roll was a harmless but violent expression of adolescent feelings, but receptive when it became controlled, conventionalized, and no longer spontaneous.

FREDERICK ELKIN

McGill University

*The Gifted Group at Mid-Life: Thirty-Five Years' Follow-Up of the Superior Child.* By LEWIS M. TERMAN and MELITA H. ODEN. Vol. V. Genetic Studies of Genius. Stanford, Calif.: Stanford University Press; London: Oxford University Press, 1959. xiii, 187 pp. \$4.50.

Since Dr. Terman's death in 1956, Melita H. Oden, his devoted assistant, has worked to complete this 35-year follow-up of students who, as 11-year-olds in 1921, had intelligence quotients over 135. These 1,500 superior students had the knowledge of their superiority reinforced by being included in the study, and many had their chances of occupational success increased by proximity to the prosperity of California and the wise counsel of Dr. Terman. That they had done well could not be seriously doubted even at the time of the 25-year follow-up. It was also apparent then that their children were not as bright as they had been. It is moreover no surprise that they continue to be a group with

good physical health, particular freedom from mental disease and divorce, and high scientific and artistic productivity. It is hard to establish a reference point from which to make this comparison, but even if one had a control set of information on less bright classmates, it is almost certain that this conclusion would stand.

It is just in the matter of refined doubting that the present volume disappoints. One still doesn't know (in the way we should know) whether intelligence relates to achievement when social class is controlled. The incredible continuity of information which these 1,500 subjects and their 2,500 offspring could provide has not been viewed as a step-by-step panel process. The cross-tabulations are only of the most rudimentary sort. And so far as I can determine, the personality data of the 1950 follow-up are not used at all.

As Sears points out in his introduction, file entries from Terman's sample can be expected to accumulate until 2010, but scientific concern for the content of the files will rest on later analysts' willingness to go beyond Terman's interests in health, adjustment, and success. To infuse new ideas into subsequent analyses will require use of judicious consultants along with continued respect for the heroic dedication that the present file and contacts represent. Virtually all longitudinal studies have fallen by the way far short of the accomplishments of this one.

Those for whom the support of long-term longitudinal research is still an open question must necessarily weigh this disappointing report in the balance. But it is also true that whatever one's position on the commencement of such studies in the future, it can be fairly argued that these data, now they are at hand, deserve continued study as a unique resource.

FRED L. STRODTBECK

*Center for Advanced Study in the Behavioral Sciences*

*Les Maternelles: Etude Sociologique sur les Institutrices des Ecoles Maternelles de la Seine.* By IDA BERGER. Préface de CHARLES BETTELHEIM, Paris: Centre National de la Recherche Scientifique, 1959. 196 pp. 1.800 fr.

This small but compact volume constitutes a report on a searching study of kindergarten teachers. Under the aegis of France's National Center for Scientific Research in 1954, 2,567 subjects working in Paris and in the Département de la Seine were contacted by means of questionnaires, 67 per cent of which were returned. A copy of the questionnaire is reprinted in the Appendix. The book yields some interesting comparative facts about the role of

teachers in France, whose socio-economic background and professional satisfactions and dissatisfactions seem to parallel those of teachers in the United States.

The first chapters of the book deal primarily with the socio-economic and geographic origins of the respondents, their parents, grandparents, husbands, and husbands' parents, as well as with the respective size of the respondents' families of orientation and procreation and the actual or anticipated occupational statuses of the respondents' children. The resultant data are then broken down and cross-tabulated, yielding information on patterns of geographic, rural-urban, and occupational mobility. Data that emerge also demonstrate the proportions of teachers whose antecedents were similarly members of the teaching professions, relations between geographic and socio-economic patterns of mobility, and, where the ages of the respondents are held constant, changes in specific patterns over periods of time. Moreover, there are data which document intra-familial mobility rates relating to both the teacher's and her husband's families and reflecting sexual differences in opportunity for mobility in France. Correlations are established between the socio-economic origins of the teachers and the occupational statuses of their husbands.

Another section of the report deals with the recruitment and aspiration levels of the subjects. The author here focuses on levels of qualification, interrupted training as well as its causes, and an analysis of the relative statuses of the original goals. The final chapters of the report attempt to deal with the question of why the respondents chose to become nursery school teachers, the degree of parental approval of their choice, and the relative degree of explicit professional satisfaction.

The book contains a great deal of information, valuable not only from the point of view of comparative education, but also to students of social mobility, the professions (career women), social mobility, social change, and demography. From the methodological point of view, it raises the question of the extent to which "occupation" is a reliable indicator of socio-economic status. Although the author's hypotheses are supported by numerical data, these are presented mostly in the form of percentages; means appear here and there but tests of significance are absent. Moreover, tables and charts are not numbered and adequate captions are lacking. This latter fact detracts to some degree from the author's clarity and conciseness of exposition.

HENRY CARSGH

*Smith College*



*Family Life in the Netherlands.* By K. ISHWARAN. The Hague: Uitgeverij Van Keulen N.V., 1959. xii, 291 pp. 45 shillings.

This work is an example of what might be termed anthropology in reverse, a type of study that will certainly proliferate as one consequence of the transformation of former Western colonies into independent nations. Dr. Ishwaran is an Indian, a product, as he puts it, of "a technically less-developed society." He obtained his "first doctorate" in ancient Indian literature at the University of Karnatak, Dharwar, and then as a mature scholar proceeded to Oxford to study anthropology and from there to the Institute of Social Studies in The Hague. The author exemplifies, of course, the modern liberal spirit in anthropology. His earnest efforts to assure the reader that the natives of the Netherlands are also human, or to lighten his text with an occasional touch of the picturesque, are a constant delight.

This is a study of the urban middle-class Dutch family and of the changes that have taken place in it over the past generation. The principal source was a long questionnaire answered by 415 students, approximately one-tenth of the student body at the University of Leiden. Intensive unstructured interviews were then carried out with 83 of the students and their families. These impressionistic data are supported by an intelligent use of census materials, other research on the family (particularly though not exclusively in Holland), and a special survey of 25 of the more important Dutch social scientists, who were asked to list "ten significant changes" in the Dutch family.

Throughout, Dr. Ishwaran is interested primarily in behavior patterns—how many meals the family eat together, how the household money is given to the mother, what foods and clothes are made at home, how work around the house is divided among mother, father, servants, and children. He asked a number of his respondents to write an account of a typical day in their lives when they were about ten years old, and these sketches (as corrected by the parents) form a base from which changes since about 1938 can be inferred. Respondents were also asked to assist in drawing kinship diagrams, which strikingly illustrate generalizations about the size of the family, the dispersion of its members both geographically and socially, and the continuation of the extended family as a meaningful unit. As a supplement to this behavioral base, questions were also put on attitudes—what did the respondents think of birth control, mixed marriage, love as the only legitimate basis for marriage, and so on. Mercifully, there is a complete absence of both

psychoanalytic jargon and of the notion that personality formation ceases at the end of toilet-training.

Family sociology in the Netherlands is largely derivative from that in the United States. Sociologists there know the theories of Ogburn, Burgess, Zimmerman, and the others; there are local versions of the propositions that the family is losing its functions, that it is becoming a companionship, that it is degenerating through polysyllabic types into utter ruin. Dr. Ishwaran is familiar with these works, and many of his findings could have been fitted into one or another of these frames.

But in the cross-cultural perspective built into this study, the Dutch family is seen as unchanged in its essentials. "Far from being unstable, [it] is in a state of equilibrium, relative to the conditions of modern life." With respect to the main functions of procreation, protection, and socialization, the family performs as well as it did in the past; and in the author's judgment the ties among extended kin retain some significance even among widely dispersed urbanites. This "optimism" certainly has a better empirical foundation than the contrary view—except when we consider Holland's population growth, where the adjustment to modern life has been deficient. For some inexplicable reason, Dr. Ishwaran discusses the birth rate only up to 1948; the sharp decline from immediately after the war to that year gives a completely false impression of the fertility during the following twelve years.

In general, I would recommend this work both as an introduction to a fascinating but little known society and, more broadly, as a thoroughly competent and intelligent analysis of one Western family type.

WILLIAM PETERSEN

University of California, Berkeley

*Africa: Its People and Their Culture History.*

By GEORGE PETER MURDOCK. New York: McGraw-Hill Book Co., 1959. xiii, 456 pp. \$8.75.

On the basis of an impressive review of the literature, Professor Murdock has written a synthetic ethnography of Africa which will replace, for many purposes, Baumann and Westermann's *Peuples de l'Afrique*. His geographical coverage is wider than theirs for he includes all the pre-colonial inhabitants of the continent both north and south of the Sahara. His selection of topics, on the other hand, is more limited for he deals primarily with "food-producing activities, the division of labour by sex, housing and settlement patterns, kinship and marriage, the forms of social and political organization,

and a few miscellanea such as cannibalism and genital mutilations" (p. viii).

The book offers both a comprehensive classification of African peoples (the index of tribal names contains some 6,000 entries) and an attempt to reconstruct the cultural history of the continent. The grouping is made "on the basis of cultural similarities and geographical contiguity." When language is being employed, the method is clear, but the weighting given to other criteria is never explicitly stated. In view of the author's sometimes idiosyncratic use of established categories (for example, Twi and Akan) this makes for certain difficulties, which are further increased by a number of factual errors (that is, the identification of Wala and Wile, the inclusion of Dagari in the Grusi language cluster).

Murdock is of course not concerned only to classify. The uses he makes of his data are "historical" rather than sociological. Indeed recent Africanists are chided for their failure to treat their data in this manner; he speaks of the valuable insights "unattainable by those . . . who confine themselves arbitrarily to purely synchronic methods of structural and functional interpretations."

Murdock certainly calls attention to a neglected aspect of African studies, especially in his analyses of the distribution of cultivated plants. Of greatest interest here is his isolation of a western Sudanic complex (sorghum, bulrush millet, cotton, etc.) which he suggests probably represents an independent invention of agriculture on the upper Niger "before it had diffused from Asia to the lower Nile, though doubtless later than its earliest development in the Near East" (p. 68), that is, circa 5000 B.C. Undoubtedly a number of plants were first domesticated in Africa, though the main authorities give no support to his hypothesis concerning cotton (Hutchinson, Silow and Stephens, 1947). But where in Africa, is another question. For sorghum, Kordofan seems more likely than the upper Niger (S. H. Evelyn, *World Crops*, 1951). Indeed this bold speculation is put forward in a manner which little suggests the absence of archaeological confirmation and of detailed genetic studies. All in all, it seems advisable, for the time being, to regard the technique of agriculture as an import from the northeast which was then applied to various local species.

The author's attempt to establish historical sequences extends to social organization. His thesis is that most of Negro Africa formerly had matrilineal institutions which have been replaced gradually by patrilineal descent, often attributed to the acquisition of cattle and its

use in bride-wealth payments. A change from matriliney to patriliney was of course an underlying assumption behind the work of many earlier ethnographers. Murdock's debt to the same tradition, mediated by Sumner and Keller, is evident in the considerable reliance he places on any traces such writers claim to find of "matrilineal survivals." This leads him to reject Radcliffe-Brown's "structural" interpretation (1924) of the mother's brother-sister's son relationship among the Bathonga, which like Junod he regards as an index of an earlier state of the society. This conclusion he describes as "sound historical scholarship" in contrast to "untrammeled sociological speculation" (p. 378).

Whatever changes in the descent system of these societies may have taken place, Murdock's choice of evidence leaves something to be desired. That something is a greater understanding of comparative sociology, which appears not only in his treatment of so-called matrilineal survivals but also in his attempt to see the majority of African states as "absolute monarchies" essentially comparable to Wittfogel's oriental despotisms, with a common origin in Egypt.

Murdock's comprehensive ethnographic treatise will prove a useful sourcebook and will perhaps encourage others not only to pursue the study of Africa's recorded history but also to reconstruct her undocumented past. Murdock has made a useful beginning. It is to be regretted that a certain animus against sociologically oriented anthropology and its practitioners, discernible, for example, in the omission of any reference to the contributions of Radcliffe-Brown in the chapter on kinship, has had such limiting effects even on the historical side of his work. The extent of this is illustrated in his remarks on the Bedouin, the chapter on whom he concludes with the suggestion that "the principal contributions of the North African Arabs to Western civilization may well be the Jewish ghetto and the inferior and segregated status of the Negro race" (p. 403). So history, far from being bunk, may serve to shore up our own shortcomings. There could hardly be a better example of the need for a careful sociological understanding before engaging in conjectural history.

JACK GOODY

*Center for Advanced Study in the Behavioral Sciences*

*A Community in the Andes: Problems and Progress in Muquiyaayo.* By RICHARD N. ADAMS. The American Ethnological Society. Seattle: University of Washington Press, 1959. xiv, 251 pp. \$4.75.

This is a description and history of a small agricultural town in the Central Sierras of Peru.

The late Wendell Bennett interested Adams in a field study there as the basis for his Ph.D. dissertation. The task set was to discover the factors that made Muquiyauyo a particularly progressive community. The town's problems were common to a wide area, but it had grappled with them somewhat more effectively than its neighbors.

Following a regional pattern, its population (2268 in 1950) doubled in the last century, outgrowing land resources. In discussing local cultural and social dynamics, Adams points to population increase as the major factor. In Muquiyauyo it forced a steady contact with the outside world since it obliged many townspeople to leave to find work. For decades they went principally to the mines. When they returned home, they brought new ideas with them. There were repeated local attempts to ameliorate the community situation with reallocations of land and public works. Close cooperation of Indians and mestizos on town projects undermined caste divisions. An unusually effective town government built an electric light plant and sponsored other improvements ahead of other similar communities.

The question why Muquiyauyo had this special bent toward "progress" proved unanswerable. But Adams amassed a mountain of field data. His year in Peru was clearly one of ceaseless industry, and in this book he spares us no detail of what he found. From the names of the local governing officials of the smallest land divisions in 1728-1735 to the precise dimensions of the adobe bricks used for house construction—it is all there. We learn that in 1932 the town had one barber, but that in 1950 there were three. This microscopic miscellany is not unusual in community studies. Lacking a clear "problem" there is no test by which any material can be excluded or even emphasized. All-inclusiveness becomes the aim, and a fortuitous weighting occurs in favor of information that is readily available. It must be said for Adams that he has carefully classified his accumulation of facts, and presents us with an informational mine.

But when he turns to his larger theme, to "show how local history is directed through the presence of a few pressing unsolved problems," he invites the question of the relevance of some of the irresistibly recorded details, and calls attention to the banality of his broad conclusions: "Recognition of a problem and the exertion of efforts to solve it do not necessarily bring success. . . . In the process of solving one problem, new problems are often created."

SALLY FALK MOORE

Vestal, New York

*A Survey of Social Conditions in England and Wales As Illustrated by Statistics.* By A. M. CARR-SAUNDERS, D. CARADOG JONES, and C. A. MOSER. New York: Oxford University Press, 1958. xxi, 302 pp. \$4.00.

*The Changing Social Structure of England and Wales, 1871-1951.* By DAVID C. MARSH. New York: Humanities Press, 1958. xiv, 266 pp. \$5.50.

These two books have a similar and specific purpose: to portray the social conditions of England and Wales by means of statistics, drawn principally from official sources. Both volumes are essentially descriptive, and emphasize social conditions rather than social structure. Carr-Saunders and his colleagues, presenting us with an up-to-date version of an earlier classic in the field—*A Survey of the Social Structure of England and Wales*—decided that "Social Conditions" was a more appropriate title since "in recent years the term social structure has come to be used by sociologists in a special sense." And although Marsh uses the term, he defines it in a special way to mean "those aspects of social life associated with the composition, distribution and divisions of the population which are capable of quantitative measurement." The dividing line between sociography and sociology is a thin one, however, and both books are to some extent analyses of the changes in English social structure which their quantitative data reflect. Marsh takes a long view based on 1871—a starting point significant in so many ways for the institutional origins of contemporary society—while Carr-Saunders aims at presenting a picture of social conditions "in the light of the latest statistics, adding comparisons with past years when illuminating and appropriate."

The presentation of general social statistics is presumably directed at the undergraduate and general reader. Here the Carr-Saunders volume clearly scores. It is a model for this kind of exposition: lucid, balanced, with a subtle momentum of its own that makes it much more than the usual compendium. Marsh, dealing with a longer time span in fewer pages and constrained by fairly intractable problems of comparison, is less successful in avoiding those textual densities and discontinuities that make for less readability. However, his book is the only one of its kind and the guidelines it provides should be of considerable help to anyone who wishes to go to the sources and make more detailed comparisons of his own.

As regards content, certain limits are obviously set by the availability of reliable statistics, and in both books government sources form the backbone of the materials. Their ar-

rangements of chapter headings are basically similar, progressing from demographic facts, through divisions according to region, industry, occupation, income and education, to such matters as religion and crime. Within this scheme, their emphases vary, and naturally their particular selectivities will not suit everyone. Nevertheless, even within their social statistical frame of reference, some of the omissions struck the reviewer as rather peculiar. For instance, Carr-Saunders and his colleagues, devoting an approximately equal amount of space to a wide array of topics in nineteen separate chapters, have facts on trade unionism under the heading of "Protective Associations" but do not see fit to bring together the available statistics on industrial relations as a whole. Similarly, they have chapters on the use of leisure time and religion but make no mention of politics, a gap which surely cannot be accounted for in terms of the comparative availability and reliability of sources in these three fields. Marsh, for his part, appears to concentrate on those areas where his long-range comparisons can be attempted. As a result, the chapters on regional variations and occupational distribution each take up twice as much space as the two on social classes and educational opportunity (VIII) and the distribution of wealth (IX) combined. For two reasons, this seems to be an unnecessary disproportion. First, because many of his comparisons are actually confined to the twentieth century material; second, because there is a good deal of information on educational opportunity during this period which he leaves out altogether. One final point of criticism is that both books are weakest when trying to present social class in statistical terms. This is no doubt due to the fact that the concept of social class, unlike their other descriptive categories, is a sociological and not a social statistical entity (a distinction, incidentally, which was implicitly recognized in the original Carr-Saunders volume). It is, therefore, an essentially arbitrary element in their universe of discourse and could just as well be made to serve as an articulating link, binding together their data as a whole, as be confined, as it actually is, to the phenomenon of occupationally based strata. But the former course would have led them to write entirely different books than the ones they have produced. Perhaps as a result of using them, as they deserve to be used, in undergraduate and extra-mural teaching, some sociologist will be inspired to do exactly that.

DAVID LOCKWOOD

*London School of Economics and Political Science*

*The Small Group.* By MICHAEL S. OLMSTED. New York: Random House, 1959. 158 pp. \$1.35, paper.

This is an ambitious little book which seeks to be useful to both the initiate and the expert by providing a frame of reference for theory and data about the small group. On the whole it is successful in this goal. The book is organized around "three sets of distinctions: the distinction between sociological and psychological emphasis; between personality, culture and social structure; and between primary-expressive and secondary-instrumental behavior." This is an interesting change from the more familiar simple dichotomy of structure and function, but like all frames of reference, it is sometimes a Procrustean bed in which the subject matter has to be stretched or compressed a bit to make it fit.

There are, the author points out, two traditions in the study of small groups: an essentially sociological one which is concerned primarily with "*society as groups*," and a newer psychological approach oriented toward the "*group as a society*." The sophisticated reader is likely to become restive as the author reviews sociologists' early concern with the taxonomy of groups and examines some studies of small groups in slums, factory, business, and military organizations. However, apart from its function of familiarizing the novice with some important studies (and incidentally establishing the priority of the sociologist's interest in the small group), a major contribution of these chapters is that their focus on natural groups, such as the family and peer group, may help correct the growing tendency (unfortunately not limited to psychologists) to confuse the group with temporary aggregates of college undergraduates herded into small-groups laboratories. The discussion of the primary group's role in authoritarian and democratic societies is interesting but seems somewhat out of place, and consumes space that might better have been used to elaborate on the role of the primary group in the socialization process, a group function which is only briefly examined.

The chapters which treat with the group as a social system (about two-thirds of the book) are generally excellent and can be read with profit by anyone irrespective of his expertise. Beginning with a focus upon the individual in the group, the author examines the impact of the group upon the member's motivations, values, and perceptions. Here the experiments of Sherif, Asch, and the Lewin group are described and critically evaluated. Turning to the group as a whole the book discusses the importance of culture for group functioning by examining the group problem-solving process,



beginning with the early "together-apart" studies (one of which dates back to the late 1890s—the experimental study of the small group is not so recent a phenomenon as some small-group enthusiasts believe), and including a reference to the recent studies of decision-making in business organizations. Under the heading of social structure are considered such topics as communication network and role differentiation, primarily leadership formation. Also examined critically here are three general theories, namely, the Lewinian group dynamics approach, the work of Homans, and of Bales. Of the three, Bales fares best by far, though none escapes unscathed. And weaving throughout the book is a discussion of

the two basic problems which every group must continually solve: its relationship to the external world as it seeks to accomplish specific tasks (secondary-instrumental behavior); and the management of tensions and the provision of positive affect (expressive-secondary behavior) so that the survival of the group may be ensured.

Olmsted is no proselytizer. His critical comments may not gain converts to the study of small groups, but they are lucid, useful, and—not the least of the merits of this book—presented in exceptionally readable prose.

BERNARD C. ROSEN

University of Connecticut

## BOOK NOTES

*Demographic Yearbook, 1958.* By UNITED NATIONS, DEPARTMENT OF ECONOMIC AND SOCIAL AFFAIRS. Tenth Issue. Special Topic: Marriage and Divorce Statistics. New York: United Nations, 1958. viii, 541 pp. International Documents Service, Columbia University Press. \$6.50, paper.

*Multilingual Demographic Dictionary.* By UNITED NATIONS, DEPARTMENT OF ECONOMIC AND SOCIAL AFFAIRS. Population Studies, No. 29. Prepared by the Demographic Dictionary Committee of the International Union for the Scientific Study of Population. English Section. New York: United Nations, 1959. viii, 77 pp. International Documents Service, Columbia University Press. Fifty cents, paper.

*The Materials of Demography: A Selected and Annotated Bibliography.* By HOPE T. ELDRIDGE. Foreword by DUDLEY KIRK. Published for International Union for the Scientific Study of Population and Population Association of America. New York: Columbia University Press, 1959. xi, 222 pp. No price indicated, paper.

These volumes furnish further evidence in support of the contention that demography is among the best organized and best documented branches of the social sciences. The first is the most recent in the annual series of admirable compilations of population data prepared by the United Nations. In this issue the subject selected for special emphasis is "Marriage and Divorce Statistics." Among the 31 tables in the volume, some 15 are devoted to aspects of this subject, in many cases to topics for

which data have never previously appeared in the yearbook series.

The *Multilingual Demographic Dictionary* is the result of several years of work by demographers in different countries. The English edition is paralleled in the first instance by French and Spanish editions, which are to be followed by editions in Russian and other languages. The dictionary is organized not as an alphabet of terms, but as a series of chapters that incorporate the technical words of demography into a running text. All the terms defined are listed in an index and are numbered so that the corresponding words in the several languages may be related to each other. The volume should prove useful not only to persons who want to translate demographic texts from one language to another, but to persons who wish, in a single language, to use with precision the vocabulary of demographic studies.

*The Materials of Demography*, commissioned by the Committee on Demographic Instruction of the International Union for the Scientific Study of Population, is intended particularly as an aid to building up library resources in demography. Over 400 works are listed, many of them journals or collections of papers among which those of most interest are individually cited. In all, nearly a thousand authors are represented. Persons starting new libraries in demography will find this volume indispensable, but those responsible for existing collections will also find it a highly valuable check upon the state of their holdings. Many users will be particularly grateful to the author for indicating the titles, somewhat fewer than 100 in number, that she considers to be of outstanding importance.—G. F. M.

*Economic Characteristics of International Migrants: Statistics for Selected Countries, 1918-1954.* By UNITED NATIONS, DEPARTMENT OF ECONOMIC AND SOCIAL AFFAIRS. Population Studies, No. 12. New York: International Documents Service, Columbia University Press, 1959. xv, 314 pp. \$3.50, paper.

The body of this book is a group of tables showing information on international migrants classified by occupation and other economic characteristics. The availability of data, the resources available for the preparation of the study, and the importance of and interest in European migration led to a concentration on material relating to migration from and within Europe. National tables are presented for thirty-six countries, almost all in Europe and the Americas. Because of the different data collection and reporting procedures of the several countries the tables given are not uniform from one country to another. Where data were not available, no estimates have been attempted, but some adjustments in classification have been made for the sake of comparability. This volume should prove highly useful to persons interested in international migration who do not have the opportunity or the time to consult original sources. Moreover, it should facilitate the improvement of migration statistics as to coverage and comparability.—G. F. M.

*International Repertory of Institutions Conducting Population Studies.* By UNESCO. Reports and Papers in the Social Sciences. No. 11. New York: UNESCO, 1959. 240 pp. International Documents Service, Columbia University Press. \$2.50.

This first attempt to compile a list of all institutions conducting studies in the population field is based on the replies received to questionnaires sent out to institutions with demographers on their staffs or believed for other reasons to be engaged in demographic research. Over four hundred institutions are listed. The information given for each varies from the simple comment that no demographic research is being conducted to a report on structure, administrative and professional staff, financial support, research activities, cooperation with other institutions, teaching and training programs, use of basic demographic data, and publications. If it receives sufficiently wide distribution, the volume should be useful to persons not working in the population field who would like to know where to obtain data on population questions. Future editions would be made more convenient through the elimination of institutions having nothing to report and the deletion of extensive lists of publications.—G. F. M.

*The World's Metropolitan Areas.* By INTERNATIONAL URBAN RESEARCH. Berkeley and Los Angeles: University of California Press, 1959. vii, 115 pp. \$3.00.

This useful reference work is the initial attempt of IUR to overcome the lack of comparative data on cities. Data presented are limited to a list of the world's metropolitan areas of 100,000 or more people, together with their populations. The latter are given for about 1955 and for one earlier date, both for the metropolitan area and for the central city. In 21 countries where metropolitan areas could not be demarcated, populations appear only for cities in the 100,000-and-over class. A lengthy appendix indicates both the sources of population data and the component parts of each metropolitan area. These areas have been delimited by the IUR staff on a scheme which is a modification of the definition of Standard Metropolitan Area used in the 1950 United States *Census of Population*.

The procedures which the staff applied uniformly are explained in the two introductory chapters, which also discuss some of the difficulties of obtaining comparable statistics under widely varying conditions. It is concluded that, for statistical purposes, the theoretically preferable concept of "urbanized area" is pragmatically inferior to the concept of "metropolitan area."

Despite problems of incomplete coverage and of inevitable variation in the reliability of specific figures, this compilation of basic metropolitan-area data is as impressive as it is welcome.—VINCENT HEATH WHITNEY

*Community Structure and Analysis.* Edited by MARVIN B. SUSSMAN. New York: Thomas Y. Crowell, 1959. x, 454 pp. \$6.50.

Any man who tries to put a reasonably well-organized book together out of the contributions of some two dozen authors, whose work runs the gamut from community typology to spectatoritis, is bound to have a job on his hands. *E pluribus unum* is an ideal seldom achieved under these circumstances. However, in this book I think that we may give the editor credit for a nice try.

His method is to give us a general introduction in which he lays out the relationships of the parts with considerable clarity and sometimes an almost deceptive smoothness. In addition, at the head of each contribution is a specific introduction, a page or two in length, in which he indicates what is to come and ties it in further with what has gone before. This is nothing new, but it is gratifying to see it done well.

The contributors, with a couple of exceptions, have supported their editor properly with chap-

ters of high quality. Their work is not only well suited to class use, but also is of interest to scholars, community workers, and the several types of planners. The book's best use would probably be as supplementary reading in a community course. However, if one must use a text, this is an engaging candidate for the job.—  
BULKELEY SMITH, JR.

*School and Community in the Tropics.* By T. R. BATTEN. New York: Oxford University Press, 1959. v, 177 pp. \$2.00.

Writing within a context more familiar to the British than to Americans, Mr. Batten addresses himself to the teacher, administrator, and extension worker in the tropics. He suggests methods by which the schools, in becoming more community-oriented, can play a more effective part in educating people to accept and understand the changes in their society. Drawing on experiences in India, the Philippines, Africa, and South Dakota, he describes the school as a social work agency and the teachers as social workers. The difficulties stemming from demanding too much of overworked teachers operating under far from ideal circumstances, the obligation of making the authoritarian structure of the school democratic, and the delicate task of convincing parents that the community school is not second-class education, force Mr. Batten to conclude that the school cannot do the job alone. He believes that the teacher must and should be a cooperative member of the team of community development workers, thereby contributing to the sustaining value of any program.—

ADELAIDE C. HILL

*The Child, The Parent, and The State.* By JAMES BRYANT CONANT. Cambridge: Harvard University Press, 1959. vi, 211 pp. \$3.50.

In this essay Mr. Conant faces facts, past and present, and recommends action to the American public. He has studied the history of the high school as a part of our evolving democracy, and he finds it to have accomplished much. He has been inside high schools, and he finds their problems knotty, their service to quality inadequate, but their major structural elements sound. He applies his empirical wits to his data and concludes that the American comprehensive public high school is likely to continue to contribute vigorously to "the preservation of the vitality of a society of free men." These are his closing words.

Right or wrong in this specific recommendation or that, the book is alive, because it calls for perception and experiment, not lambasting or debate. Mr. Conant asks his fellow citizens

to define their corporate role in improving the work of the high school of their communities, bids them face the practical problems of quality within equality and warns them that they are going to have to dig into the state and federal purses for the necessary money. The book deserves the tough-minded study that went into its writing.—CLIFFORD R. BRAGDON

*The Child's World: His Social Perception.* By FRANK J. ESTVAN and ELIZABETH W. ESTVAN. New York: G. P. Putnam's Sons, 1959. xiii, 302 pp. \$4.95.

The basic assumption of the present volume is that what children do and say and believe is a consequence of the way they perceive their world and the events that make it live. The authors emphasize particularly how the "perversity" of adults in ignoring the facts of the children's world affects the recommendations which adults make about the content of the school curriculum.

The main body of the volume consists of the application of projective techniques designed to enable adults to learn about the ways children perceive situations in which people are central. Three types of situations are dealt with: rural and urban, social status, and child and adult. Two concluding chapters summarize the study and its significance.

Perception, it is concluded, is not unitary behavior, but "is influenced by a number of factors which interact in different ways to produce a wide range of individual differences" (p. 264). Among these are physical factors, language, experience, class status, sex, intelligence—all "testimony of the inseparable relationship between perceiving and living" (p. 277), which means, among other things, that "perception may continue to develop for as long as an individual lives."

This book merits careful reading, both for the techniques utilized and for the implications of its conclusions.—JAMES H. S. BOSSARD

*Sociometry in the Classroom.* By NORMAN E. GRONLUND. New York: Harper & Bros., 1959. xviii, 340 pp. \$4.50.

*Sociometry in Group Relations.* Revised Edition. By HELEN HALL JENNINGS. Prepared for the American Council on Education. Washington, D. C.: The Council, 1959. xi, 105 pp. \$1.50, paper.

Both these volumes are intended to help the teacher understand and use sociometric techniques in the classroom. Both have a practical rather than a theoretical orientation and both are heavily laden with implicit, and often ex-

PLICIT, value judgments concerning the goals of the teacher and the school and of desirable pupil behavior in the classroom.

However, Gronlund goes beyond Jennings by including extensive reviews of relevant research, thereby contributing to a synthesis of research findings. The limitations of sociometric procedures, and cautions in interpreting findings, are more extensively treated by Gronlund; Jennings, on the other hand, is more enthusiastic than objective.

As a quick rundown of sociometric procedures and the purposes for which such techniques can be used, Jennings is perhaps too brief for the teacher unversed in the approach and oversimplified for the sophisticate. It is unlikely that this volume will contribute to the author's goal of "enhancing careful, focused sociometric work in the classroom" (p. vii).

Gronlund's book, while it does not always escape the major pitfalls of methodological naivete and value-laden biases afflicting sociometric studies, is generally more subdued and reasoned in the evaluation of sociometric procedures as diagnostic techniques and the basing of "therapeutic" attempts on such findings.—  
GEORGE PSATHAS

*An Overview of Adult Education Research.* By EDMUND DE S. BRUNNER, DAVID S. WILDER, CORINNE KIRCHNER, and JOHN S. NEWBERRY, JR. Chicago: Adult Education Association of the U.S.A., 1959. viii, 274 pp. \$4.00.

This volume surveys the research relevant to the processes of non-vocational adult education. The topics range from adult learning through motivation, attitudes, participation, methods, and leadership to group dynamics and community. The authors eliminated purely descriptive studies from consideration and gave primary attention to research which provided a base for further testable hypotheses. They have accomplished the task they set for themselves in a commendable manner. Social scientists as well as students of education will find this volume valuable both as a review of present knowledge and as a delineation of the major areas in which research is needed.

Scholars with special competence in the specific areas discussed will find points on which to take issue as well as weaknesses. Since each of the several authors wrote separate chapters, there is some overlapping and variation in the way topics are presented. The index is quite inadequate and there is no bibliography, but the book's contributions to research in the field far outweigh its deficiencies.—

WILBUR B. BROOKOVER

*An Introduction to Electronic Data Processing.* By ROGER NETT and STANLEY A. HETZLER. Glencoe, Ill.: Free Press, 1959. 287 pp. \$6.75.

Perhaps five years ago this book might have been a useful but undistinguished guide to business management on computers. However, present accomplishments in the computer field so surpass the primitive conditions described in this book that glorifying these old course notes as a book now must be deplored as irresponsible publishing. In addition to being seriously out of date, the book is marred by numerous errors in physics, in facts, and in language.

The computer field has at least two kinds of relevance to sociology. There is the use that may be made of computing in data reduction and in the simulation of social systems (which is feasible with this new tool). It is seldom, however, that a sociologist gets into the position of contracting for and staffing a computer operation; hence, he has little need for the service this book attempts to offer. Rather, he uses what is available, and it is a pity this book does not tell him more on this score.

The second relevance is the study of the computer movement itself. If there are any good sociological studies of this important contemporary development, they have escaped the reviewer. Especially worthy of study is the organized cooperation that exists among the users of certain equipment. If someone were to undertake a study of this field, it should be rewarding. Such a researcher might even be able to explain why this book was thought to be needed.—  
IAN C. ROSS

*Bureaucracy, Aristocracy and Autocracy: The Prussian Experience 1660-1815.* By HANS ROSENBERG. Cambridge: Harvard University Press, 1958. xi, 247 pp. \$5.00.

Bureaucracy is accepted by historians and sociologists alike as a fundamental theme in the development of Western society. In recent years a number of studies have come out which throw light on one of the most formidable bureaucracies of modern Europe, that of Prussia. Rosenberg has not merely written a history of the Prussian bureaucracy, but has examined its relation to the crown as well as to the Junker squirearchy; he thus succeeds in throwing light on the connections between administration and politics, bureaucracy and class structure.

In Prussia, as elsewhere in Europe, the dynastic absolutism of the 17th century was replaced in the 18th and 19th centuries by a bureaucratic absolutism. However, in Prussia bureaucratic rule was built up on a working alliance with the Junkers. The effect of the alliance between bu-



reocracy and aristocracy was not the abolishment of privilege but at best the broadening of privilege to include some *homines novos*, the beginning of a process which has been called the "feudalization" of the German middle classes.

In his postscript Rosenberg sketches with conviction, even passion, the relevance of his scholarly findings to the development of Germany in the 20th century. But his thesis that there is a direct connection between a tradition of autocratic government and the occurrence of totalitarianism deserves more careful discussion than we find in the crowded last pages of the book. While it can be argued that, on the whole, the German bureaucracy put up no effective resistance against National Socialism, it cannot be denied that there was some innate resistance to authority (cf. Rosenberg's own discussion of the "resistance fighters" and "inner emigrants" among the Prussian bureaucrats in the days of Frederick II).

As a whole Professor Rosenberg's excellent book constitutes a case study of inestimable value for those who are concerned with the history and sociology of the modern bureaucracy. —KLEMENS VON KLEMPERER

*Social and Cultural Mobility.* Containing complete reprints of *Social Mobility* and Chap. V from Vol. IV of *Social and Cultural Dynamics*. By PITIRIM A. SOROKIN. Glencoe, Ill.: Free Press, 1959. 645 pp. No price indicated.

The reissue of *Social Mobility* (1927) provokes the question "How much have we developed beyond what Sorokin was able to do a generation ago?" This query invites comparison with Lipset and Bendix's *Social Mobility in Industrial Society*. The data of the latter are firmer and more intensive than those available to Sorokin. The latter ranged through all societies and historical time to adduce evidence about the generic nature of mobility, while Lipset and Bendix concentrate on a limited number of industrial nations. The two volumes have a similar focus, seeing mobility in multi-dimensional terms and as fluctuating in character and rate at different times in the history of a nation.

Wherein then do they differ? It seems to me that the chief gain of the years since Sorokin is that Lipset and Bendix analyze data in relation to hypotheses in a more refined way. Sorokin provides illustrations to support hypotheses; Bendix and Lipset not only see deviant cases as ways of sharpening and elaborating hypotheses but systematically analyze bodies of material to sift out commonalities and dynamics.

*Social Mobility* is well worth reading again today. It is surprisingly modern and, as we

develop new foci in studies of mobility, its modernity will probably continue to unfold. The chapter from *Social and Cultural Dynamics* (1941) is entitled "Genesis, Multiplication, Mobility, and Diffusion of Sociocultural Phenomena in Space" and is now most likely to interest students of the sociology of arts and communication and mass culture.—S. M. MILLER

*The Human Side of Urban Renewal.* By MARTIN MILLSPAUGH and GURNEY BRECKENFELD. Baltimore: Fight-Blight, 1958. viii, 233 pp. No price indicated, paper.

This journalistic account of attempts to restore deteriorating urban neighborhoods in Baltimore, Miami, and Chicago should be of some interest to sociologists, despite the scientific limitations inherent in the very nature of the effort. Since few urban sociologists have given systematic attention to problems of urban renewal, these summaries of measures taken, and of achievements, failures, and problems will help to fill, in a small way, the present substantial gap in our knowledge. Although many of the "facts" that are reported are based only upon the impressions of presumably knowledgeable observers, and the conclusions are for the most part summaries of "the things that can (and have) happened," the authors do suggest many of the factors that enter into the complex problems of urban renewal: racial differences, political interests, community sentiment and neighborhood morale, housing needs, costs, taxes, education, local leadership, professional intervention and participation, operations of real estate agents, landlords' resistance, home ownership, population change. But for more reliable analysis of the importance of these variables and their relations to one another we must wait for systematic research by social scientists rather than the often enlightening but still limited accounts by sophisticated journalists.—E. C.

*How Cities Grew: The Historical Sociology of Cities.* By JEAN COMHAIRE and WERNER J. CAHNMAN. Madison, N. J.: Florham Park Press, 1959. xiv, 141 pp. \$1.95, paper.

Sociologists today rarely use historical data, which sometimes provide much better insight into problems than insignificant figures derived from "microsociological" investigations. Particularly in urban sociology, the historical antecedents are sorely neglected, perhaps because the American cities are very young and did not experience the political changes which so often decided the fate of the Old World cities. Comhaire and Cahnman have provided us with an

all too brief "Historical Sociology of Cities." The subject deserves much more extensive treatment but obviously no publisher would dare to print a large volume which has no chance to be sold with profit.

The book consists of two parts. The first, written by Cahnman, deals with the significance of urban history, gives an historical typology, outlines the stratification in cities, and concludes with a discussion of ecological factors. The larger second part, written by Comhaire, is divided into four sections (Ancient Cities, Medieval Western, Modern Western, and Non-Western Cities), followed by "General Conclusions." The scholarship of the authors is attested by the fact that they have drawn from sources in seventeen languages, a feat which few will be able to duplicate. Thus the book covers the entire field, but limited by space (136 pages of text) the authors had to confine their presentation to basic facts. There are occasionally some slight inaccuracies. It is also possible to differ with some of the interpretations. For instance, Cahnman's objections to the theory of the origin of cities as fortresses (p. 2) are not convincing and, in part, are contradicted by Comhaire's data on Arab cities (p. 114). But minor faults are to be found in every book. They do not detract from the fact that we have here a unique contribution of great interest and value. One may wish that the authors will have the opportunity to treat the same subject again on a much larger scale. As it is, the booklet is still a fascinating account of urban dynamics.—

EGON E. BERGEL

*What Is Political Philosophy?: And Other Studies.* By LEO STRAUSS. Glencoe, Ill.: Free Press, 1959. 315 pp. \$6.00.

This volume consists of ten studies which are devoted to such diverse subjects as Xenophon's *Hiero*, Maimonides' ideas about political science, Locke's doctrine of natural law, and the author's views about the proper role of political philosophy. All of the studies have been published before.

Of most interest to the social scientist will undoubtedly be the essays on the nature of political philosophy. According to Strauss, political philosophy is in a state of decay, and the reasons are to be found in the general acceptance of "social science positivism" and "historicism." The former is blamed for the refusal to make value judgments and the depreciation of prescientific knowledge; the latter is charged with insisting on the historically conditioned character of all philosophical issues and therefore rejecting the very question of the good society. As against these false doctrines, Strauss praises the classical philosophy of Plato and Aristotle which sought to replace opinion about the nature of political things by knowledge of the nature of political things and attempted "truly to know . . . the right, or the good, political order."

Those familiar with Strauss' writings will not be surprised to learn that this work lacks a systematic discussion of how the dichotomy of fact and value might be overcome and how we might attain knowledge of "the good."

GUENTER LEWY

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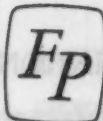
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